# Contemporary Psychology

#### A JOURNAL OF REVIEWS

	100 100 100 100	and T. S. Krawiec Reviewed by S. C. ERICKSEN
December 1961 VOLUME VI - NUMBER 13	419	Delinquency and Opportunity: A Theory of Delinquent Gangs, by R. A. Cloward and L. E. Ohlin Reviewed by F. L. ROUKE
	420	Rorschach Psychology, by Maria A. Rickers-Ovsiankina (Ed.) Reviewed by L. F. Shaffen
	422	Education and the Human Quest, by H. A. Thelen Reviewed by GLEN HEATHERS
	423	Development of the Perceptual World, by C. M. Solley and Gardner Murphy Reviewed by M. A. WALLACH and LISE WALLACH
	425	The Central Nervous System and Behavior, by Mary A. B. Brazier (Ed.) Reviewed by A. J. RIOPELLE
	426	The Strategy of Conflict, by T. C. Schelling Reviewed by K. E. BOULDING
	428	CP SPEAKS By the Editor
	430	Word and Object, by W. V. O. Quine Reviewed by E. H. GALANTER
	432	The Presentation of the Self in Everyday Life, by Erving Goffman Reviewed by D. R. Millen
	435	State in Language by T. A. Saback (Ed.)

(Continued on inside ower)

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#### CONTENTS-continued

436 Characteristics of Teachers: Their Description, Comparison, and Appraisal, by D. G. Ryans Reviewed by D. A. Prescott

438 The Rorschach Experiment: Ventures in Blind Diagnosis, by S. J. Beck
Reviewed by Martin Mayman

441 Social Behavior: Its Elementary Forms, by G. C. Homans

Reviewed by T. M. Newcomb

442 The Chemical Basis of Clinical Psychology, by A. Hoffer and Humphry Osmond Reviewed by J. C. Pollard

The Social Epidemiology of Mental Disorders: A
Psychiatric Survey of Texas, by E. G. Jaco
Reviewed by R. N. Wilson

Visual Field Defects after Penetrating Missile Wounds of the Brain, by Hans-Lukas Teuber, W. S. Battersby, and M. B. Bender Reviewed by Mortimer Mishkin

448 Chronic Schizophrenia, by Lawrence Appleby, J. M. Scher, and John Cumming (Eds.)
The Out-Patient Treatment of Schizophrenia, by S. C. Scher and H. R. Davis (Eds.)
Reviewed by D. D. GLAD

450 Personality and Social Encounter: Selected Essays, by G. W. Allport

Reviewed by O. A. OESER

451 Perspectives in Psychological Theory: Essays in Honor of Heinz Werner, by Bernard Kaplan and Seymour Wapner (Eds.)

Reviewed by A. L. Baldwin

452 Social Rehabilitation of the Subnormal, by H. C. Gunzburg

Reviewed by Seymour Sarason

453 Blue Collar Man: Patterns of Dual Allegiance in Industry, by T. V. Purcell. Reviewed by R. L. Kahn

454 The Dynamics of Psychiatric Drug Therapy, by G. J. Sarwer-Foner (Ed.) Reviewed by R. E. EDWARDS

456 Psychoanalytic Concepts of Depression, by Myer Mendelson
Reviewed by Elizabeth R. Zetzel

457 INSTRUCTIONAL MEDIA

Edited by A. A. Lumsdaine UNESCO's bibliography on the influence of films on children, reviewed by Eleanon E. Maccoby

459 Name Index 465 Subject Index

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## Have the Systems Receded into History?

J. P. Chaplin and T. S. Krawiec

Systems and Theories of Psychology. New York: Holt, Rinehart and Winston, 1960. Pp. xviii + 473. \$6.50.

Reviewed by STANFORD C. ERICKSEN

Chaplin started in the West and then went East as he made good. His AB was at the University of New Mexico, his PhD at the University of Illinois, and now he is at the University of Vermont as chairman of the Department of Psychology. His co-author, Krawiec, stays put in the East: AB at Colby, MA at Brown, PhD at New York Uni-, versity. He is also somewhat of a world traveler although chairman of the Department of Psychology at Skidmore College. Both men have published in the popular vein as well as on the solid side. The reviewer, Ericksen, is Professor of Psychology at Vanderbilt University, where he has just completed fifteen years as head of the department. He watched it grow from a two-man department to a full-fledged graduate outfit. Just now he is on leave at the University of Michigan's Mental Health Research Institute, where he is thinking about thinking and working on problems of conceptual transfer. He is active in APA affairs and is President of the Southeastern Psychological Associa-

Tost psychologists, forty and over, have grown up with the unquestioned assumption that one's primary mission is to search for the systematizing principles which will reduce the diversity and strengthen the conceptual unity of psychology's vast discipline! When I look up from the pages of this new book, however, and view the shortrange and long-range goal-strivings of my fellow psychologists, on campus and off. I am suddenly reminded that this conceptual Crusade may have quietly ended and New Frontiers have been added. These authors seem less concerned and have proceeded in a direct and forthright manner to write a contemporary version of the Grand Tradition-the scholarly procession of Great Ideas moving with measured tread across the psychological scene, à la Boring Heidbreder, Woodworth, and Murphy, Still this volume was not designed to be an echo of these masters; it is a dignified and self-sufficient treatise written in a good, gray style by two faithful reporters. They did not try to make over psychology or to introduce new systematizing concepts.

It is easy to feel that the old books are the best books, but when we 'cor-

rect' for the growing complexity and materialism of psychology, we must recognize this volume as being an excellent contribution to an overlooked (or bypassed) textbook area. The authors are not presumptuous nor are they naive, and they must surely realize that their content material, like training, ethics, and the importance-of-doing-research. represents a subject matter about which most of us feel some degree of expertness. I would advise the authors not to be disturbed by the various vested voices that will complain about the inadequate treatment given to a particular, specialinterest area. These authors have tackled what needed doing in a period when professional affairs have strong dominance and when most research psychologists seem to be turning their attention to specific experiments, programed research programs, and "miniature theories" (to use a phrase oft repeated in this book).

In preparing to write this book, the authors' first major decision was to go backward in psychological time until they found seven different "mental processes" that remain as active and important aspects of theoretical psychology: sensation, perception, learning, thinking, motivation, feeling and emotion, quantitative psychology, and personality. These are their chapter headings, and each one is written as a self-contained unit. This means that, like Heidbreder's Seven Psychologies published 28 years ago, it still seems best, if not necessary, to 'package' psychology in at least seven different categories and with little formal attempt to tie them together.

For the most part, these 'content' chapters are presented in a fairly stand-



J. P. CHAPLIN

ard pattern. The authors' usual procedure was to select a well-known reference written by one of the early systematicists, like Titchener's Textbook of Psychology, Carr's Psychology: A Study of Mental Activity, Hull's Principles of Behavior. Insofar as the selected representatives had anything to say about the given topics, these same references were repeated. The writers were careful not to introduce new authors and points of view without good justification. It might have been easier to quote Titchener, Carr, Watson, Freud. etc., but the writers chose, quite wisely, to introduce their own paraphrasing and condensations and thereby maintain the continuity of ideas at a level and at the pace appropriate for the newer student in psychology. The instructor using this text will probably want to add some of the 'real-life' flavor which can make systematic and theoretical psychology more interesting. The hot theoretical issues of the earlier days have cooled off and remain in these pages as intellectual differences of opinion between serious and well-meaning but impersonalized men. Much of the drama of our conceptual history has been precipitated out.

Perhaps modern textbooks are written with a view toward planned obsolescence in the manner of automobiles and household appliances. Nevertheless,

the frequent use of the heading "contemporary . . ." may have been a tactical error since the material referred to will be far from "contemporary" to students reading the book in 1965. In fact, many such topics could already be classed as historical; viz., Current Trends in the Psychology of Thinking is limited to the 1941 publications of Goldstein and Scheerer, the 1934 and 1935 articles by Jacobsen, Hull's 1920 monograph on concept formation, and Heidbreder's 1946, 1947, and 1948 studies on types of concepts. World War II is already history to most college students and for them "contemporary trends" applies to events starting with the Kennedy administration.

There are, nevertheless, some excellent summarizing statements throughout this book which should be carefully considered. Note, for example, the statement near the end of the treatment on learning: "With some exceptions, today's learning theorists no longer seek a magic formula to explain learning in general, but instead strive to define types of learning and formulate their theories accordingly. Thus, the 'miniature' theories have grown increasingly important to the point where they have displaced the older global theories. . . . But no matter how 'microscopic' theories may eventually become, someone, at some time, must once again bring together the subtheories in a 'macroscopic' account of learning as a whole' (p. 269). As a matter of fact, the sequential build-up used throughout the successive chapters may, for the serious student, generate enough momentum to lead him to ask the instructor, "what happened next; what have been the significant theoretical developments in the past ten years in perception, learning. thinking, personality, etc.?" This is an excellent characteristic of the book and one which defines a rather definite, and often difficult, contribution by the teacher-to help the student establish the conceptual continuity between the high points in the historical panorama and the available current reviews in the several specialized areas.

Throughout the book different readers will find different places where they will be tempted to argue with the

writers as to which topics best represent the systematic development of a given major area. The authors are aware of this sampling limitation and we should not take strong exception to their we-are-trying-to-be-eclectic position. As a matter of fact, if there is a selective bias, it is the faithful reflection of psychology's almost stereotyped repetition of a limited range of historical events; we need some new exemplar references to illustrate and to document our growth toward maturity. The repetitious use of the same names-Titchener, Carr, Watson, Hull, Freud, Köhler, etc.-tends to focus attention on the man rather than the systematic concepts and theories, per se.

This book was not written exclusively for members of the APA and the standards of internal consistency and conceptual scope and technical refinement that one might ask of the Koch series. Psychology, A Study of a Science, would be quite out of place for a text for undergraduates and first-year graduate students. The enterprise achieves its planned objective very well—to abstract and to present in some sequential arrangement the more enduring principles that provide the prerequisite background for systematic and theoretical psychology today.

Even so, two major questions remain: First, at what distance should



T. S. KRAWIEC

one stand to see the Big Picture of systematic and theoretical psychology? The chapters start out at a rather distant point where we first see the dominant features that were established by a few early experiments and ideas. These then become completed in greater detail as the text moves us in for a close-up of the first half of the twentieth century in psychology. By the end of each chapter, one may feel that he is looking at a 'guess what' cover photograph for Science; we can see the Figures but what is the Ground? Perhaps there is a built-in contradiction between trying to sample almost 100 years of theoretical output and still provide the systematic continuity from past to present.

A haunting thought continues to disquiet this reviewer as question two: has the 'locus of control' been transferred from the systematically thinking psychologist to a hungry but rich and service-demanding society? This contemporary book on systems had little to add by way of integration and theoretical unity, but, if this attenuation is to be criticized, it is not the fault of the authors but a direct reflection of the changing emphasis in psychology. Perhaps this predicament means merely that the sociologists or other Zeitgeist specialists will, in the near future, be the ones to write the interpretive and perspective account of the Big Picture in psychology.

What we have here is a new book wrestling with an old problem. The problem remains after the wrestlers have quit. This state of affairs probably means that the broad, systematic issues are not as important as they were once upon a time.

#### Delinquency's Subcultures

Richard A. Cloward and Lloyd E. Ohlin

Delinquency and Opportunity: A Theory of Delinquent Gangs. Glencoe, Ill.: Free Press, 1960. Pp. x + 220, \$4,00.

Reviewed by Fabian L. Rouke

Cloward is Associate Professor of Sociology and Ohlin Professor of Sociology. both in the New York School of Social Work, Cloward received his graduate training in sociology at Columbia, Ohlin at Indiana University and the University of Chicago, where he was for some time the Director of the Center for Education and Research in Correction. They are cooperating now in the direction of research in delinquency and penology. The reviewer, Rouke, is head of the Psychology Department in Manhattan College in New York City. He publishes in the field of delinquency and criminal motivation. He has served in a delinquent school and in the U.S. Naval Prison, worked for the Society for the Prevention of Crime, treated shoplifters in department stores, acted as consultant to the police in lie detection. He is a founder of the Manhattan College Institute for Forensic Research and belongs to the steering committee of the Metropolitan Law Enforcement Conference.

These authors set out to develop a theory that will explain the conditions which result in a subcultural rather than individualistic solution to problems of adjustment. They are not concerned with the psychological mechanisms that influence the development of an individual delinquent but concentrate on the social pressures that affect delinquency as a collective phenomenon. They define three types of delinquent subcultures-the criminal, the conflict and the retreatist-and are interested in those forms of delinquent activity which result from the performance of social roles specifically provided and supported by such delinquent subcultures. The criminal subculture is devoted to illegal means of securing income, like extortion or theft, and may serve as a 'minor league' for the ranks of professional criminals. The conflict subculture has violence as its hallmark and is the source of neighborhood gang battles. The retreatist subculture is restricted by the authors to include only those delinquencies that involve the use of drugs. This is the subculture of the 'cool cats.'

The authors start with two questions: "(1) Why do delinquent 'norms,' or rules of conduct develop?—(2) What are the conditions which account for the distinctive content of various systems of delinquent norms—such as those prescribing violence or theft or drug use?" They acknowledge the theoretical perspective of Emile Durkheim and Robert K. Merton in their orientation to the first question, and the developments from the work of Clifford R. Shaw, Henry D. McKay, and Edwin H. Sutherland in their approach to the second.

They use commendable scientific caution when they note that the evidence upon which their theory of the distribution of delinquent subcultures is based is "fragmentary, impressionistic and uncoordinated," and that very little of it is available to substantiate the facts of distribution in the large cities or in rural areas and small towns. They also are careful to indicate that conclusions about the subculture distributions are assumed to be accurate not because of strong supporting information but because of the absence of contradictory information. Ultimately they present a well-developed series of arguments to show that the individual motivation of delinquent behavior is only a part of the picture and that the actual type of deviant behavior indulged in is also partly a function of the culture in which the individual is living.

They present and discuss critically several theories of delinquent subcultures. Among them are the theory of masculine identification offered by Talcott Parsons and also by Albert Cohen and Walter B. Miller, theories which consider that delinquent subcultures stem from the transition of adolescents into adulthood, like Herbert Bloch's and Arthur Neiderhoffer's, and finally Walter

B. Miller's variant of the traditional culture-conflict theory which attributes the emergence of delinquent and criminal subcultures to value conflicts. They also discuss critically the contributions of Louis Wirth and Thorsten Sellin.

Special attention is given to Emile Durkheim's concept of 'anomie' (law-lessness or normlessness) and to the thinking of Robert K. Merton in his advancement of Durkheim's work. From these sources evolves the authors' basic hypothesis.

Our Hypothesis can be summarized as follows: The disparity between what lower-class youth are led to want and what is actually available to them is the source of a major problem of adjustment. Adolescents who form delinquent subcultures, we suggest, have internalized an emphasis upon conventional goals. Faced with limitations on legitimate avenues of access to these goals, and unable to revise their aspirations downward, they experience intense frustrations; the exploration of nonconformist alternatives may be the result.

CLOWARD AND OHLIN develop their hypothesis systematically, with free reference to the literature of sociology with which they are patently familiar. Their references to studies of delinquency by investigators in psychiatry and clinical psychology are limited. The psychological references that appear are mainly from experimentalists in the field of social perception. The few clinical studies mentioned are of but one approach, the psychoanalytic.

The authors advance their hypothesis to the belief that each individual occupies a position in both legitimate and illegitimate opportunity structures. This is their concept of differential opportunity which unites recognition of differential access to both legitimate and illegitimate means of achieving goals. They hold that the kind of support given for a particular type of illegitimate activity at different points in the social structure determines the way in which the delinquent will resolve his problems and also determines toward which subculture the delinquent will move. A neighborhood characterized by close bonds between offenders of different age levels and between criminal and conventional elements will nourish the criminal subculture. The conflict subculture emerges where all opportunity, either criminal or conventional, is frustrated, and where there exist conditions of relative detachment from all institutionalized systems of opportunity and social control. "Double failure," failure in the use of both legitimate and illegitimate means, is the milieu of the retreatist subculture.

Cloward and Ohlin attribute the stability and persistence of a delinquent subculture to its ability to recruit new members. Once in existence it may be a force to attract individuals to the deliquent life by serving as a means of need fulfillment. When that happens, the individual may internalize the norms and values of the group and become a stable member. In the criminal subculture delinquents tend to be integrated with adult offenders and the subculture tends to persist. Conflict subcultures are relatively unstable because they are not fostered by the interest of even the criminal adult. The retreatist subculture, since it has integration with the criminal subculture-at least for continuing the drug supply-is moderately resistant to change.

In their closing two paragraphs the authors take the extreme position that "delinquency is not, in the final analysis, a property of individuals or even subcultures; it is a property of the social systems in which these individuals and groups are enmeshed. . . . It is our view, in other words, that the major effect of those who wish to eliminate delinquency should be directed to the reorganization of slum communities." This conclusion does not seem justified on the basis of the argument, especially since the authors themselves admit that the evidence for their argument is so fragmentary.

The strongest reaction of the reviewer to this book is that it emphasizes the need for an interdisciplinary, rather than the single-discipline approach, to the problem of delinquency.

#### U

I have always had a Fancy that learning might be made a Play and Recreation to children.

-JOHN LOCKE

## The Blot's Escutcheon

Maria A. Rickers-Ovsiankina (Ed.)

Rorschach Psychology. New York: John Wiley & Sons, 1960. Pp. xvi + 483. \$8.50.

Reviewed by LAURANCE F. SHAFFER

The editor, Dr. Rickers-Ovsiankina, is Professor of Psychology and Director of Graduate Training in Clinical Psychology at the University of Connecticut. She received her PhD from the University of Giessen in 1928 and was soon afterward a research psychologist at the Memorial Foundation for Neuro-Endocrine Research at Worcester, Massachusetts. She has long been associated with research on the projective methods, is the author of Rorschach Scoring Samples (1938), and was the president of the Rorschach Institute in 1943-44. In this volume she is supported by sixteen co-authors, all of them able and several eminent. The reviewer, Dr. Shaffer, is Professor and head of the program in clinical psychology at Teachers College, Columbia University. He was editor of the Journal of Consulting Psychology from 1947 to 1958 and president of the American Psychological Association in 1953. For fifteen years he has conducted a graduate seminar on research in methods for psychological appraisal. He has used the Rorschach but says that he finds himself more of a friendly skeptic than one of the faithful.

Rorschach has developed as a clinical technique in substantial isolation from the main streams of psychological theory and research. The past decade—in contrast, however—has seen an emerging interest in the relationships between perception and personality. The present volume is therefore an inevitable product of its time, an attempt to view the Rorschach in terms of general and special psychological theories. The editor and her sixteen collaborators have avoided the production

of yet another instructional or clinical manual. They direct their appeal "to theory-minded readers from the ranks of clinicians and general psychologists alike."

What is the general theory of the Rorschach? In an introductory chapter the editor sets the propositions of a projective, holistic, and Gestalt psychology. Like personality itself, the Rorschach is a configuration in which each part interacts with each other. The person can be understood from the total pattern that his total self imposes on the unstructured materials. Such a statement is theory in one sense, but perhaps not in the exact sense used by scientific disciplines. This fundamental 'theory' of the Rorschach and of personality is more a set of postulates whose acceptance is assumed or sought, rather than a sequence of related hypotheses to be tested by crucial evi-

In addition to general theory, many of these chapters present theories of varying degrees of specificity. Among them the sensory-tonic theory of Heinz Werner and Seymour Wapner offers the clearest bridge between general psychology and some aspects of the Rorschach. The good chapter on developmental theory (Laurence Hemmendinger) draws extensively from Werner. That on the experience balance (J. L. Singer) uses Werner and Wapner's concepts of the relations between sensory and motor processes. Another theory of great generality, psychoanalysis, is not so well represented. It appears, meagerly and not too successfully, only in a chapter on assessing primary and secondary process through the Rorschach content (R. R. Holt and Joan Havel). The chapter on the color response (David Shapiro) offers a theory that presents the generality that color perceptions, unlike those of form or movement, are spontaneous, unmediated, and therefore more primitive or emotional.

The other 'theories' are more Rorschach-bound. The chapter on organizational activity (Marguerite R. Hertz) is a scholarly and useful summary of Rorschach methods but does not draw on other approaches to cognitive functions. Similarly particular are the treatments of form perception (S. J. Korchin), of

M and flexor and extensor M (Z. A. Piotrowski), and of an unusual interpretation of the white-space response (C. P. Fonda). The general psychology of perception seems to hold some ground in common with Rorschach psychology, yet it is rather hard to bring them together.

Every theory that deserves the designation should produce a series of testable hypotheses which generate empirical research. All contributors (except the two European ones) accept the intimate relationship between theory and evidence and cite from 19 to 118 references, mainly to research studies. The amount of empirical research is impressive, but its quality often is not. A reviewer should treat a slip or two with charitable silence, but the shortcomings are numerous enough to require a warning that the reader should examine with a critical eye the original articles cited.

The chapter on the white-space response says, "The most clear-cut findings show that the higher a person's rate of S emission, the more likely he is to be bright (21 . . .) . . . ingenious (21 ...), and self-sufficient (21)" (p. 93). The thrice cited article designated by "(21)" reported, among other data, the correlations of 37 non-Rorschach variables with S. Two of these 37 correlations were significant, about what one would expect by chance. The original authors wisely drew no positive conclusion. The present chapter extracts two conclusions, "bright" and "ingenious" from an r of .197 with PMA reasoning when five other PMA scores are not significantly related, and a conclusion of "self-sufficient" from an r of -.251 with the number of sociometric choices made.

The chapter on form perception, in support of the proposition that good form is a measure of ego strength, cites a nonsignificant r as positive evidence, and gives in some detail the findings of an apparently positive study not confirmed by two replications (p. 118). An article cited as evidence for the meaning of flexor and extensor M suffers from such severe inconsistencies in its tables as to raise grave doubts about its competency (p. 137). In another instance, a finding on the internal-con-

sistency reliability of the Rorschach is reported without qualification as "an average correlation of .85 for three groups" (p. 371) when the original research was concerned only with the reliability of R, the number of responses.

Two chapters give results of their authors' original studies not reported fully elsewhere. Neither presentation would have passed an editorial screen. Two reported studies of the movement response are post hoc analyses lacking cross-validation (pp. 139-141). An elaborate study of the scoring of content for primary and secondary processes seemed strikingly confirmed for a group of 15 men and not confirmed at. all for a group of women (pp. 311-314). Do the results show a sex difference, or a failure of cross-validation that negates the method? Only further research can answer that question, and, pending the emergence of such evidence, the publication of 40 pages of detailed scoring instructions has perhaps been premature.

Another striking characteristic of the book is the scarcity of negative evidence until the final chapter. A reader not already well informed might receive the impression that almost all research confirms the positions presented by the authors. Rorschach research is complex and much of it is conflicting. Many of the generalizations would be challenged by worthy studies that are not even listed in the bibliographies or the name index.

 $\Gamma_{
m wo}$  chapters near the end of the volume supply the book's most substantial contributions. The reviewer engaged in a fantasy of what might have happened if these chapters had been written first and made required reading for the other contributors. Lois and Gardner Murphy, after a sensitive appraisal of Hermann Rorschach's place in the development of psychological thought, develop a theory of considerable generality in terms of levels, functions, and the interdependence of functions. To sample all these might require a formidable instrument capable of evoking hundreds of responses. Rorschach's unique insight was that a single response might give multiple information about several levels and functions; but this attractive economy,

basic to the widespread clinical use of the instrument, has its hazards for a genuine understanding of personality through research: "... sampling so much with so little gets one involved in contamination, in problems of low reliabilities which cannot be explained away . . ." (p. 353). Rorschach's pioneering work might evoke three attitudes, write the Murphys, to revere him, to standardize and codify him, or to push on to new accomplishments which require a blending of earlier and contemporary methods. The authors favor the third course, which demands the development of new techniques that retain the merits and correct the faults of the Rorschach.

A similar conclusion is reached in the long and useful final chapter on validity, by Jesse G. Harris, Jr. Here at last, many technical problems of the Rorschach receive the frank recognition they demand—the language of the examinee and examiner, the sufficiency of sampling, the nature of criteria, and the effects of variations in conditions, materials, and examiners. The chapter looks to the future. The Rorschach is neither as dead as its opponents aver, nor as eternal as its faithful adherents believe. It is a prelude to research on personality that has hardly begun.

Rorschach Psychology is an honest representation of the contemporary state of its field, with all of its inspirations and confusions, all of its good and bad theory and research. As such it is a successful book that will stimulate both thoughts and feelings in the informed and critical reader and in the graduate student under appropriate guidance. It deserves to be read.

## The School Involves the Community

Herbert A. Thelen

Education and the Human Quest. New York: Harper & Brothers, 1960. Pp. 224, \$4.75.

Reviewed by GLEN HEATHERS

The author, Dr. Thelen, last year at the Center for Advanced Study in the Behavioral Sciences, is Professor of Education at the University of Chicago and the author of Dynamics of Groups at Work (Univ. Chicago Press, 1954). He has taught natural science at high schools sponsored by the University of California and later the University of Chicago. In 1951 his community program for Chicago's South Side won him an award and the review notes that the book under review has had special commendation by the American Library Association. The reviewer, Dr. Heathers, is Professor of Education and Director of the Experimental Teaching Center of New York University. He has been a staff member of the Social Science Research Council, Director of the World Citizenship Study of the National Coun-

cil of the VMCAs, and a Research Associate of the Fels Research Institute for the Study of Human Development.

THE NOTABLE BOOKS COUNCIL of the American Library Association recently selected this volume as one of 46 "notable books of 1960." This selection is understandable for the book should appeal as much to laymen as to professional educators. It is written in a lively, nontechnical style, with copious examples. It offers a basic approach to educational change that challenges those who place their faith in demanding more from students, in strengthening the curriculum, in TV, in teaching machines, or in team teaching. It calls for vigorous action to reduce "the fifty-year lag of education behind the social sciHere is a new exposition, and a new defense, of what has come to be called progressive education. Though John Dewey is mentioned only in passing, his stamp is unmistakable in the emphases given the process of inquiry, problems of living, and education for democracy. What is added beyond Dewey is mainly the technology of group dynamics. The author also brings to his task a substantial body of ideas and methods he has acquired during fifteen years of research on social processes.

Thelen seeks to present both a theory of education and a program of action for the improvement of education. His theory of education, better called a 'viewpoint,' relates conceptions of the individual, knowledge, and society. His notion of psychodynamics is, to say the least, out of date. In responding to stress, he says, the individual may employ either reflex behavior or inquiry. His list of 'reflexes'-fight, flight, immobilization, dependency, and seeking intimacy-confuses the unlearned with the learned. His assertion that the most fundamental human need is for autonomy can be debated. Perhaps autonomy should be everyone's basic goal: but, if indeed it is, how many people keep that fact well hidden!

The chapter, Knowledge: The Instinct for the Jugular, gives us, I suppose, Thelen's razor. Ducking the analogy, we note that he distinguishes knowledge from mere information by noting that it involves 'meaning' for the individual and depends on inquiry. The reader will look in vain for a systematic account of the process of inquiry, for the author appears to be more concerned with the settings within which inquiry occurs and with the functions it serves for the individual and the group.

Thelen offers "personal inquiry" as the road to autonomy and to an individual Weltanschauung. Such inquiry is a blend of individual study by the project method and of self-study, aided by individual and group counseling. He considers personal inquiry important but places more stress on inquiry in group contexts.

It may surprise the reader that "group investigation," not individual study, should be presented as the main way of acquiring knowledge, and that group investigation is justified as necessary to get students to acquire knowledge for which they have no immediate use. Thelen evidently does not have great faith in students' learning for the love of learning. Since he believes both in social learning and in the autonomous learner, it would have been helpful had he clarified how the two are related.

PERHAPS the most valuable portion of the book deals with the "reflective action" that involves group inquiry in learning to make wise decisions on matters of policy and on the technological applications of knowledge. This enterprise calls for the concerted efforts of parents, school administrators, teachers, and students in working toward the educational goals that, presumably, they hold in common. The school becomes a laboratory for planning and action to improve education. Living and learning at school become integrated with living and learning at home and in the neighborhood.

This conception of educational change as a function of the total community deserves serious attention as an alternative to the relatively fragmentary, relatively authoritarian, attitudes that now prevail. A decade ago, an historic attempt to apply this approach was made at Antioch College under the leadership of Douglas McGregor. Thelen reports a successful project of this nature in his home community in Chicago. In his chapter entitled. Do It Yourself: the Greatest Project, he proposes that each community establish a "Citizens Education Council" to blend the virtues of inquiry and organization in planning and conducting educational change.

In his proposals for educational change. Thelen leaves some large questions unanswered. One concerns curriculum. Who is to decide what is to be taught? The author decries the highly-structured curriculum; yet one bane of education under the banner of progressivism has been its tendency to lack structure in respect of what is to be taught, and to whom, and when, and how. One cannot be sure that, by letting students learn what they want to learn, or by having local school leaders or parents decide curricular contents

and sequences, the highest achievements of education will have been served.

In our democracy, the interests of state or nation often take precedence over the interests of the particular locality. How does Thelen stand on the present concern of our national government for strengthening the teaching of science, mathematics, and foreign language? Should there be a National Curriculum Commission, as Paul Hanna proposes?

A closely related question concerns leadership for educational change, Training for leadership is vigorously emphasized in the movement of group-dynamics, and it was considered at length in Thelen's earlier volume, *Dynamics of Groups at Work*. In the present volume, he does not discuss the roles to be

played by experts in the different subject matters, by professors of education, or by school administrators. As a university professor, does Thelen perhaps foster educational change only in the role of a 'resource person' or a 'consultant,' or does he truly assume a more active role in initiating and conducting new projects?

Certainly the professional educator will find many stimulating ideas in this book. Particularly noteworthy are the author's approach to setting up "teachable groups," his proposal for a "Skill Dvelopment Laboratory," and his views on resolving problems of the adolescent by providing for school-age youths more significant roles in school and community.

#### Liberal Eclecticism for Perception

Charles M. Solley and Gardner Murphy

Development of the Perceptual World. New York: Basic Books, 1960. Pp. xiv + 353. \$6.50.

Reviewed by Michael A. Wallach and Lise Wallach

Gardner Murphy, as everybody knows, is in these days Director of Research at the Menninger Foundation and the author of many scholarly books, of which one of the more recent is Human Potentialities (Basic Books, 1958; CP June 1959, 4, 161-164). Solley, a PhD from the University of Illinois, was a participant in the Perceptual Learning Project at the Menninger Foundation under Murphy. He is now Assistant Professor of Psychology at Wayne State University in Detroit. He writes on cognitive processes and motivation. Michael Wallach, a Swarthmore AB, a Harvard PhD and once an instructor there, late a fellow at Cambridge University, is now Assistant Professor of Psychology at the Massachusetts Institute of Technology. See his review of Thomson's Psychology of Thinking (Penguin, 1959; CP, July 1961, 6, 246f.). Lise Wallach, a Kansas PhD under Fritz Heider and Alfred Baldwin and later instructor at Bryn Mawr, is now Assistant Professor of Psychology at Wellesley College. She is working on concept formation which she thinks is a kind of perceptual learning.

THE aim of this book can best be indicated in the authors' own words: "Scattered throughout the varied literature, including developmental and experimental studies, theoretical treatises, psychoanalytical and psychiatric papers and books, there is rich, fertile, but unorganized information about how learning (usually under the influence of motivation) influences the molar components of the perceptual act. This information is so scattered and disorganized that few people know of its existence or scope. One might say that the major aim of this book, its raison d'être, is to pull together and integrate this information into a theoretically consistent frame of reference, incomplete though it may be" (p. 26).

This is certainly an impressive goal. The authors have shown courage and an all too rare sense of the unity of psychology in assembling between two covers discussions of research, ranging from reports on psychiatric patients who were overwhelmed with painful memories when rigid muscles became relaxed, through experiments on figure-ground dominance, to Russian work on the orienting reflex.

Solley and Murphy build their interpretation around the view that perceiving is an act not unlike motor responding. This act they analyze into a temporally organized sequence of psychological events: expectancy, attending, reception, trial-and-check, and the percept—with autonomic and proprioceptive arousal considered as both influenced by and influencing the later stages. Each of these events reveals the effects of experience and motivation, and it is the authors' intention to formulate an integrated theory concerning the ways in which these effects operate.

Expectancy is thought to reflect the environmental probabilities which the S has experienced, but also to be modified by motives. Attending is conceived of as an act of orientation which can be conditioned through reward training.

Into reception, learning is said to enter in two principal ways: through habituation, where repetition of a stimulus leads to a decrease in its ability to elicit response, and through sensitization, where the opposite of habituation occurs. The authors provide, unfortunately, no specification as to when one or the other ought to obtain. Sensitization is assumed to cover the sensory integration of Birch and Bitterman, the Gibsons' concept of differentiation, and sensory preconditioning. It is not entirely clear, however, that inclusion of all these three lines of work under one concept is warranted, for, while sensory integration and sensory preconditioning can be conceptualized in terms of S-S learning or some other type of an associative process of enrichment, it was exactly the Gibsons' contention that this could not be done with the phenomena to which they were drawing attention.

Nor are the authors always as careful as one would wish in analyzing evidence. For example, they present Ivo Kohler's experiments as supporting their view that percepts become conditioned to

particular response patterns. In one experiment that they report, the Ss wear glasses in which the right half of each plate-glass eye-piece is yellow and the left half blue. Upon removing these glasses, after wearing them 20 to 60 days, the authors say that eye-movements to the right lead to the perception of yellow while eye-movements to the left lead to the perception of blue. Kohler, however, actually found the opposite-eye-movements to the right lead to the perception of blue, and those to the left lead to yellow. The Innsbruck studies, rather than showing the conditioning of percepts themselves to motor movements, have demonstrated the occurrence of long-term perceptual adaptations that can become dependent upon such movements, as well as upon other variables.

Expectancy, attending, and reception do not directly lead to the percept itself. There is next a stage of trial-and-check in which the sensory stimuli are tentatively assimilated into mnemonic schemata with which they may or may not be congruent. If not sufficiently congruent, another trial-and-check ensues.

THE selection of evidence at times seems somewhat arbitrary. Thus, in considering the effects of autonomic and proprioceptive arousal, it is proposed that these internal signals can function to stabilize or distort the percept or the trial-and-check process; yet the major perceptual evidence cited in support of this contention is a study by Snyder with only five subjects. Given a rectangle of fixed width, S's task on each trial is to adjust its height until the rectangle appears square. On some trials, if S makes the adjustment below a certain height and calls the figure square. he receives a shock (without awareness of the relation between height and shock). While initially there was a strong tendency to underset the height (i.e., to call the figure square when too short), succeeding trials led to a systematic increase in height settings up to the point where these approached veridicality and then overshot equality.

Apart from the fact of the small sample and the fact that this type of experiment, as we are told, has been discontinued, the lack of a control condi-

tion, with the same experiment repeated without shock, makes it difficult to accept Snyder's results as evidence for the effects of internal signals on perception. The tendency to set the square too short when practice begins may simply be an example of the vertical-horizontal illusion, which consists in overestimation of the vertical as compared with the horizontal. With respect to the tendency for height settings to increase as a function of successive trials, we may note that practice alone, without reinforcement or knowledge of results, has been shown to decrease a variety of geometrical illusions, sometimes even leading to a reversal of the illusion.

Instead of concentrating on admittedly preliminary data, whose relevance is not entirely clear in any case, it might have been better to sample from Werner and Wapner's and from Witkin's researches, which have indeed demonstrated that, as Solley and Murphy assert, the role of internal signals in visual perceiving is extremely important.

The last stage in the sequence is represented by attainment of the organized percept itself. The authors consider as an example of this attainment the segregation of the phenomenal field into figure and ground. In general, here and elsewhere, the volume leans heavily upon a large number of studies growing out of the early experiment by Schafer and Murphy. On the whole these studies indicate that, in an ambiguous figureground situation, rewarded profiles tend to be reported as figure and punished profiles as ground. Many exceptions to this generalization are presented, however, particularly with regard to the role of punishment. It is noted, for example, that a number of experiments on clarity of perception and perceptual thresholds. as well as on figure-ground, support the proposition that punished material will be perceived more readily if the conditions of presentation suggest to S that escape from or avoidance of the punishment is possible. Research also is described where, although no escape or avoidance is possible, the punished material is still perceived more readily. The authors state, in connection with some of this latter work, that punishment may act as an 'emphasizer; but the conditions under which such emphasis should be expected are not clear.

There remains the further problem of the extent to which the measures described really indicate changes in perception over and above changes in response tendencies. The authors, early in the book, point with incisiveness to the need for converging operations as a necessary means for insuring that the effects observed are in fact perceptual. As one interesting suggestion for a converging operation of this kind, they note that "if a subject reports that one of two alternatives is 'figure' and the other is 'ground,' then stimulus generalization should occur along the dimensions of the figure more than along those of the ground" (p. 17). No such converging operations were used in the studies reported, however, and the absence of such operations does not seem to be taken into account in the interpretation of these studies.

A salient virtue of this volume is its sensitivity to the history of the field and its attempt to place diverse findings in a common perspective. In their desire to integrate, however, the authors fall prey on occasion to fashioning lists of points of view instead of striving for syntheses. This seems to be the case, for instance, in their discussions of autism and schemata. Although their goal seems to be an explanatory theory and they suggest some explanatory mechanisms in the early part of the book, these are seldom used in the interpretations of experiments. For the most part they seem to be furnishing phenomena with descriptive labels rather than giving them explanations.

Although we cannot feel that the authors' goal of a theory integrating the different points of view concerning the effects of experience on perception has been achieved, the rich variety of approaches which they have presented suggests that such a goal may, indeed, be incapable of accomplishment. What Solley and Murphy have achieved is a widely ranging review of relevant literature, a review written with a refreshing awareness of historical and clinical contexts.

#### Good Talk about the Nervous System and Behavior

Mary A. B. Brazier (Ed.)

The Central Nervous System and Behavior. (Transactions of the Third Conference, 21-24 Feb. 1960.) New York: Josiah Macy, Jr. Foundation, 1960. Pp. 475. \$7.50.

Reviewed by ARTHUR J. RIOPELLE

The editor of this book and the editor of the series is Dr. Brazier, a neurophysiologist at the Massachusetts General Hospital in Boston. She is English with a London PhD, an expert electrophysiologist and electroencephalographer, the author of Electrical Activity of the Nervous System (Macmillan, 1951). The reviewer, Dr. Riopelle, has been for two years Director of the Verkes Laboratories of Primate Biology, successor to Yerkes, Lashley, and Nissen. He is a Wisconsin PhD and established the Primate Behavior Laboratory at Emory University in Georgia, where he taught for seven years.

The conference program of the Josiah Macy, Jr., Foundation is an experiment in communication in which a few presentations are made each day by and to a small group of interested (and interesting) participants. The number of prepared presentations is reduced to a minimum in order to permit maximal group interchange. This policy is adhered to in the present conference and the virtues and vices of this approach are plainly evident.

The conferences are held because it is believed that there is only one subject matter for science, nature, that nature must be studied as a whole, and that a multidisciplinary approach is needed for understanding. Scientists, on the other hand, because of numerous reasons, such as lack of training and university departmentalization, have tunnel vision, if not myopia as well, and see only the domains in which they were trained. Naturally, the solution is to increase the amounts of communica-

tion between scientists of different disciplines and residence.

It is a fact of our scientific culture that this view is maintained at a time when numerous symposiums are held each year, interdisciplinary societies hold annual meetings with thousands in attendance, and multidisciplinary journals like Scientific American and American Scientist enjoy wide circulation. Indeed, many today decry the rise in multiply authored books and transactions of symposia and the decline of works by single individuals.

Whether or not the presumed insulation of scientists has been as deleterious or the solution as successful as has been assumed can neither be decided or proved readily. It can also be mooted whether it is better to bring scientists together to talk or to conduct research.

The belief in the salutary effect of communication between scientists of different disciplines gives meaning to the structure of the present conference. Its purpose was not to solve a problem but to raise many. Thus, few restraints were imposed on the participants or the program. The theme of this conference. maturation of the central nervous system in relation to behavior, is, accordingly, barely discernible either in most of the presentations or in the group inter changes. This is not to say that the presentations are unimportant; quite the contrary, the data presented are all good and challenging.

THE evolution of man's brain is reviewed by H. W. Magoun, Louise Darling, and J. Prost. The review is accompanied by 74 sensitively selected and

significantly annotated photographs and drawings. They raise again the unanswered question of why man's present brain size was attained by Neanderthal, man. "about 100,000 years before any significant use was made of it." They propose the fascinating notion that man's rapid intellectual advance occurred because man has been able to substitute for phylogenetic modification maturational changes in his brain acquired during post-natal life. Thus, man's intellectual advance is greatest between the ages of 4 and 20, when the brain increases by about 120 grams only, rather than during the first four years of life when it expands by almost a kilogram. Maturation of the brain's functional capacity is related instead to cytological complexity.

Eugene Roberts' section on biochemical maturation of the CNS is directed toward an analysis of the changes with age of chemical factors that are associated with the generation and conduction of nerve impulses, especially GABA (gamma-aminobutyric acid). This compound and related others are shown to increase rapidly to a steady concentration in the brain of chicks within the first few days after hatching. That fact was pounced upon by several of the participants to suggest a correlation with possible behavioral acts which appear at the same time. Temporal coincidence is, however, an unreliable basis for assigning causative basis.

The second paper dealing with ontogenetic changes in behavior is by Harry Harlow on affectional responses, the essential points of which are familiar to American psychologists. A rather jarring point raised in this discussion is Harlow's skepticism that there is any fundamental difference between maturation and the process of learning.

W. H. Bridger has attempted to take the two signaling systems of Pavlov, to show that numerous others have proposed analogous dichotomous systems, and to show further that man shares the instinctive sensory-perceptual realm (first signaling systems) with lower animals but that the second signaling system (symbolic functioning, conceptual thinking) is man's alone. Yet both systems are shaped by the culture.

In view of the felt importance of international communication, it is not surprising that foreign participants were important in the conference. The presentations of the Russians, particularly, will help acquaint American investigators with the work being carried on in the Pavlovian tradition, something almost totally abandoned in this country but which now is rapidly long revived.

A. N. Sokolov's discussion concerns the nonspecific orienting reflex and its properties and the neuronal model, which is significant for the structure of the orienting reflex and the conditioned response. The neuronal model, built up after many repetitions, is a chain of neural cells which preserve the information about the intensity, the quality, the duration, and the order of presentation of the stimuli. This concept will, to many, be similar to Hebb's concept of the cell assembly. The orienting reflex is established when the sensory input is discordant with the neuronal model. This orienting reflex increases sensory discriminability through direct neural channels and through an indirect. vascular mechanism.

A. R. Luria, who is remembered for his early studies on the role of tension in learning, has contributed a summary of his ingenious researches on the voluntary control of behavior in young children. He describes three states, of which the most revealing appears to be the second in which the semantic side of speech fails if it comes into conflict with the motor side,

The third foreign participant, R. A. Hinde, an ethologist, presents data to show that some complex stimuli evoke simultaneously approach and avoidance responses in birds.

Out of these diverse presentations a conference was made with most of the papers generating mostly relevant discussion. These published transactions will be of limited use to most investigators of behavior as a reference handbook or as a source of references to a specified topic, but the publication itself is only an incidental outcome of the conference. The principal purpose of the conference was to permit face to face conversation among scientists with different backgrounds, and that goal was fully achieved.

#### The Nightmare of Rationality

Thomas C. Schelling

The Strategy of Conflict. Cambridge, Mass.: Harvard University Press, 1960. Pp. x + 309. \$6.25.

Reviewed by Kenneth E. Boulding

The author, Schelling, is Professor of Economics at Harvard University and an Associate for the Center for International Affairs there. Two of his better known books are National Income Behavior (McGraw-Hill, 1951) and International Economics (1958, Allyn and Bacon). The reviewer, Boulding, is Professor of Economics at the University of Michigan, and the author of nearly a dozen books of which some of the more recent are The Organizational Revolution (Harper, 1953), Economic Analysis (3rd ed., Harper, 1955), The Image (Univ. Michigan Press, 1956), Principles of Economic Policy (Prentice-Hall, 1958), and Conflict and Defense (1961).

This is a brilliant and important book, which makes a significant move toward establishing a body of analytical theory in the abstract field of conflict processes. The basic tool is game theory, which is in turn an extension of the economist's attempt to build a theory of rational behavior. It is no accident, therefore, that Schelling is a professor of economics, for the kind of theory that he builds is a natural extension of the economist's way of thinking and may, therefore, be somewhat unfamiliar to the psychologist.

The field of social phenomena from which this theory abstracts, however, is not the production-and-exchange complex of the economist but the field of military relations, that is to say, the field of the relations of organizations which are specialized in conflict. Conflictual relations of this type are not, of course, confined to military organizations but are observable in some degree in almost all phases of social life—for instance, in the family, in churches,

and in firms. Military organizations (armed forces), however, have almost no other function beyond carrying on a conflictual relationship, so that the Schelling theory might quite properly claim to be a pure theory of military relations. Oddly enough, it is a theory of military relations which does not really include a theory of war itself. The world of Schelling is a world of threats, promises, commitments, deterrents, capabilities, potentials, and the like, all of which involve the use of the possibility of war in a nonwar relationship. Once war actually breaks out, one feels, the Schelling system has come to a boundary and a new type of relationship takes over, one based much more on actualities than on potentials, on performances rather than on threats. One is tempted, therefore, to call it a theory of diplomacy, as representing the relationship of hostile states at peace. This too, however, would be a little misleading, for the Schelling theory does not cover the subtleties of diplomatic discourse and tells us very little about the course of negotiation or the roads to agreement and settlement. The closest model is that of military relations, during peacetime, but without the diplomats, that is to say, without overt communications among the parties. The processes which Schelling considers are essentially reaction processes, in which one party acts, and the other reacts, and the first reacts to the second's reaction, and so ad infinitum. Agreements, if they exist, are tacit rather than explicit and consist in parallel habits of behavior on the part of the parties.

It is impossible in a brief review even to summarize the richness of theoretical system which this book contains. Though it begins with game theory, it goes far beyond the usual mathematical limits, just as economic theory in the hands of a master extends beyond the calculus of maximation. In his Appendix B, Schelling makes what is to my mind an important contribution to the pure theory of games, in pointing out the possibility, the necessity even, of assuming asymmetry in the strategies of the two parties. For the most part, however, this book is an exercise in ap-

plied game theory, rather than in the pure, and it will not necessarily please the mathematicians. The light which it throws, however, on the exact nature of threats and promises, on the importance of "saliency" in the establishment of tacit agreements, on the relationship between commitment to a position and bargaining power, and, therefore, on the paradoxical bargaining strength of the weak whose implicit threats must be believed because they have no alternatives, and on many similar aspects of social systems establishes it as a major theoretical contribution.

The very importance and virtues of this work, however, make its limitations, if they are unrecognized, all the more dangerous. As long as Schelling occupies a middle ground of applied theory and is discussing an abstract and generalized version of aspects of the empirical world, it is hard to praise his work too highly. When, however, he steps outside of this arena to give advice about the real world of international relations, then he runs the grave danger of mistaking his abstraction for the reality and of falling into disastrous error. Political and social relations are much richer and more complex than military relations. Schelling's world, rational as it pretends to be, is in reality a world of rational nightmare, devoid of "mercy, pity, peace and love," slipping into rational deceit, rational cruelty, endless and implacable rational hostility, rational despair, and rational terror. It all ends, one fears, in the rational lunacy of eventual mutual annihilation. One fears Schelling has been seduced by the Rand Corporation which he so much admires. Paul Johnson (New Statesman, 12 May 1961, p. 754) has described these mental attitudes as "pornography," especially for "able young officers with intellectual pretensions." The analogy is exact; the pornographic is that which substitutes a picture for a reality. It is desirable to have art, and it is desirable to have clear abstract theories of military relations. To mistake either of these for reality itself is disastrous.



The great basic activity is unconscious.

—F. Nietzche (1844-1890)

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Vol. 8 June, 1961

#### CONTENTS

No. 2

Applications of Genetics in Psychiatry and Neurology, Lewis A. Hurst

Some Data on Natural Fertility, Louis Henry

Predictions in Fertility, MARIA
DAVIDSON

Brief Reports and Communi-

The Connecticut Twin Registry;

A Short Course in Medical Genetics, JOHN L. FUL-LER and VICTOR A. MC-KUSICK

Book Reviews
GORDON ALLEN, Book Review Editor

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"A JOURNAL OF BOOK REVIEWS"

SIX YEARS ago CP began life with an alias. A Journal of Book Reviews, a qualifying specification that indicated the medium through which the contemporary scene in psychology would be viewed. Except for an occasional rationalized self-indulgence. CP has not ventured beyond this pale. There are many ways in which contemporary psychology can be presented and perceived, but in general CP has abided by its instructions: it has reviewed books.

There are, however, a good many different ways of reviewing books, and it seems appropriate, as CP now passes from childhood to adolescence, to examine its early years as to what it has been and especially as to what it has not been. Now, if ever, would be the time to begin working on its worse faults. Altogether CP has thought of eight ways in which it might have been different, in which a choice was made with consequences, but each of them is connected, at least indirectly, with the major question of CP's primary goal. Is it CP's chief function to purvey information, or does it rather undertake to stimulate thought, trusting that discussion and criticism will carry with them enough information to leave its readers sufficiently informed as well as stimulated? It is this second alternative that CP chose in 1955 when policy was being laid down, and now is the time to consider whether the choice was good, whether CP cheated by giving its readers too much excitement and too little peaceful fact.

For the most part CP's mail has brought in praise, ever since the first year when readers complained about the pictures, the large-type citations, the review titles, the aphorisms, and all the white space that goes along with this posh style of living, but incidental praise

is no poll, for protest usually lingers quietly below the threshold until it builds up enough pressure to explode. Last summer, however, one reader did indeed explode in his own behalf and on behalf of many other "disgruntled readers" (so he said), wishing *CP* would settle down to facts and stop talking about reviews' being inevitably idiosyncratic (*CP*, July 1961, **6**, 238f.). He knew what he wanted, this correspondent did, and, of course, he was already getting quite a lot of it, although not enough—not enough fact and too much opinion.

CP in those days used to say of itself that it was not a 'service journal.' but in this context service was not the right word. Is stimulation no service to a mind that needs excitement so badly that it builds up its own hallucinations under extreme sensory deprivation? It is for this reason that dissent, by stimulating, performs a service, as CP has just remarked (CP, Nov. 1961. 6, 395). Doubt too is good-Descartes thought it was-for its occurrence is proof that mind is alive. Said Judge Jerome Frank thirty years ago: "The acceptance of everything as transitory, the welcome of new doubts, . . . the zest of adventure in investigating the conventional-these are the life-cherishing attitudes. . . . the attitudes of the so-called scientific mind, . . . the emotionally adult or nature mind" (CP, May 1961, 6, 149).

Now what are these eight choices that CP says it made and has tried to hold to?

(1) Criticism. From the start CP told its reviewers not to abstract the books but to criticize them. A reviewer was to talk about a book and in doing so to indicate the range and nature of its content. He was to note the book's purpose and assess the author's success in achieving it. He was to place the book in perspective, historically and within the contemporary scene. Solid criticism

of this sort should carry enough information with it to satisfy the reader's need for description.

(2) Truth's servomechanism. To encourage value-judgments about books is to open the way for idiosyncracy in assessment. Even when the reviewer tries to found his assessment firmly on fact and inexorable logic, his unconscious biases will slip in and his conclusion will differ from the equally careful judgment of another critic. Plainly this difficulty results from CP's having invited criticism. CP insists that the remedy lies in repeated criticism and counter-criticism, in dissent to dissent to dissent, in the columns of ON THE OTHER HAND. With continuing rejoinder and riposte, the Spielraum within which disagreement can persist gets smaller, and agreement between dissidents, which is all that 'truth' is in this context, is approached

(3) Stimulation. It is thus in the clash and interaction of ideas that criticism and dissent produce stimulation. CP said in January 1956 that it aimed to be interesting (CP, 1, 13). No small part of the interest that it has aroused in its readers comes from this freedom to criticize and dissent. The letters in 8-point type get read as fully, perhaps even a little more carefully, than the reviews in 9-point. Criticism provides a social stimulus, and there is always interest in a contest.

(4) Responsibility. Freedom for idiosyncrasy needs to be coupled with responsibility. CP has perhaps not always kept every reviewer in line and the most bitter dissents have arisen when the first critic was blind to the book author's values or even to his stated facts. On the other hand, those who find CP's reviewing too uniformly favorable arguesome of them do-that CP's criticism is not objective enough and would be more valid if it were given anonymously, CP has steadily set its face against anonymous reviewing, choosing candid honest onymity as preferable. Anonymity can cloak prejudice and personal hostility fully as easily as onymity vields to generosity or the fear of giv-

(5) Depreciation. Every now and then a counter-critic remarks that a book, if it is really as poor as its reviewer made

out, should not have been reviewed at all. Of course CP has preferred to review the more important and the more psychological books, and the really bad books as a rule have been screened out by a succession of sieves-the author's and his friends' superego, the publisher and his readers, the editor and his reviewers. Nevertheless an important bad book ought to be reviewed and also some others at the lower level in order to keep the perspective of contemporary published psychology clear, and also because the negative judgment about the book in the first place may be idiosyncratic, needing to be corrected presently by the servomechanism of dissent.

(6) Length. CP has favored longish reviews-from 500 to 2000 words, sometimes longer for special reasons, almost never shorter except by inadvertence. It is almost impossible to be interesting by furnishing perspective and developing well-founded criticism in less than 500 words. Some length is necessary if these goals are to be gained, and CP thinks that many reviewers enjoy the freedom of being able to say their says without severe constraints. Nevertheless it is still true that the 4000-word man finds a 1500-word ceiling excruciating. There was one reviewer who tried-but in vain-to fill a 1500-word assignment with 10,000 words.

(7) Idealism. The reviewers and Consultants have all worked for CP without pay-in these days when consultants' fees can be enormous as compared with earlier academic standards. At first CP was frustrated: how could it command talent with so little to give in return? Must psychology's poverty keep CP inferior to the Saturday Review and the New York Times Book Review? During the last four years this feeling has, however, evaporated. CP believes that it has had the best talent psychology had available, even though some of psychology's most talented wise men steadfastly refuse to undertake reviewing. Psychology's best is moreover, better and more abundant than CP ever expected it to be. The pay has been found partly in prestige, but also in the fun of being involved cooperatively in a largish common enterprise. Describing the reviewers, because readers of 1956 said they did not know who the reviewers were, turned out unexpectedly to be prestige-pay. Reward has, moreover, been autocatalytic. An important talented reviewer increases *CP*'s prestige, which helps *CP* then to secure another important able reviewer. *CP* has no reason to be despondent over psychology's modern commercialism.

(8) Training. When CP was choosing its goals in 1955 it included among them a desire, somewhat diffidently expressed, to contribute to good writing among American psychologists. It asked: "Can not psychologists write well?" and commented further: "The embryo CP wondered, hoped, and was not too sure. CP means to be interesting, and interesting writing is good writing" (CP, Jan. 1956. 1, 13). This bit of CP's mission had an educational slant, and CP thinks its effort has had an effect, that American psychologists are less diffident about trying to write well than they were when C. P. Snow's "two cultures"science and humanism-were supposed to be incapable of symbiosis. There have been two processes at work. The good writers have been coming out of hiding and asking for space. The poor writers have been trying to write up to the good writers, and CP has been enormously gratified by the tolerance with which so many reviewers have accepted heavy editing, given arbitrarily in accordance with CP's own assured idiosyncrasies as to what would make the review clearer and more interesting. So the good writers have become bolder and the poor writers better, and CP looks upon its sexennium with the conviction that so much progress has now been made that American psychologists might just as well keep on along the same path. How much better is intelligibility than dubiety! And how much lovelier is fun than hard work! If it is the duty of the scientist to communicate his results, why should he not also take on the responsibility for getting his communications read, for being interesting? At any rate CP chose this mission among the others and sees no reason to believe that American psychologists cannot command the English medium-or that those who do not cannot learn.

None of these aspirations of CP's was essential. You could have a journal of

short, uncritical, anonymous, dull abstracts, and it would perform an important service function. Not everyone likes *CP* for what it has been, but *CP* has had goals, has tried hard to reach to them, has failed on occasion but succeeded on others, and now leaves the old home to live with a new editor where certainly some of the old aspirations will seem obsolete. Let the Zeitgeist control. Trust him. He has Fillmore Sanford to guide him and what more could *CP* ask?

#### THE THIRTEENTH REVIEWER

THE critics want "better" review-ers; the authors want peer reviewers; the friends of "abused" authors want multiple reviewers; and they all write as if CP were omniscient, prescient, and omnipotent, as if CP's failure to please them were merely a failure to select correctly, whereas most of CP's failures to please are due, not to its informational incompetence, but to its motivational incapacity. Some people never write reviews; some hardly ever; all are choosy; and even the avid reviewer may find the invitation out of phase with his leisure. For a long time CP's record stood at nine-eight refusals before an acceptance. There were two cases. Then it went up to ten. Now, as CP1 hands over the torch to its successor-motivator, the record stands at thirteen: twelve persons refused and then the thirteenth not only said Yes, but sent in a really good review. Wisdom is power in the sense that it is a sine qua non to success, but without motivation it is just not enough. Syllogisms do not have to end in action. When we know more about the necessary techniques, all editors can and should be hypnotists.

-E. G. B.



I have made a ceaseless effort not to ridicule, not to bewail, nor to scorn human actions, but to understand them.

(Sedulo curavi humanas actiones non ridere, non lugere, neque detestari, sed intelligere.)

-SPINOZA

#### Analysis of Language with a Behavioral Bias

Willard Van Orman Quine

Word and Object. Cambridge, Mass.: Technology Press, Massachusetts Institute of Technology; New York: John Wiley & Sons, 1960. Pp. xvi + 294. \$5.50.

Reviewed by EUGENE H. GALANTER

The author Quine-see what the reviewer says about his eminence-has been at Harvard since his summa cum laude at Oberlin in 1930, as Fellow with a PhD in 1932, as Junior Fellow in the Society of Fellows, and then much later as one of the distinguished Senior Fellows, as instructor, professor, and, since 1955, Edgar Pierce Professor of Philosophy. There have been interludes with teaching at Oxford, teaching at San Paulo, and germinating at the Center for Advanced Study in the Behavioral Sciences, the year the reviewer was also there. Quine has written more than half a dozen books on logic since 1934 and has also cooperated with other writers in essays about the philosophy of Alfred Whitehead and Rudolph Carnap-different books, of course, The reviewer, Galanter, is a Swarthmore AB with a University of Pennsylvania PhD under Nelson Goodman in 1953. He has been a Fellow at Harvard and is now Professor of Psychology in the newly organized Department of Psychology (since 1958) at the University of Pennsylvania. When at the Behavioral Center he cooperated with George Miller and Karl Pribram in writing Plans and the Structure of Behavior (Holt, 1960; CP, July 1960, 5, 209-211).

WILLARD VAN ORMAN QUINE, Edgar Pierce Professor of Philosophy at Harvard University, is one of the great analytical philosophers and logicians in the world. A new book from him is an important occasion to the professional philosopher. To the professional psychologist it is of importance only as a scholarly event, unless it promises en-

lightenment on technical issues or it bases philosophical conclusions on psychological facts or theories. Although the first few chapters would appear to do the latter, I maintain that they do not, but rather that they clarify certain important technical questions. Specifically, Quine demonstrates that a common form of psychological inquiry yields indeterminate results.

The object of the book is to explore the philosophical implications of a detailed analysis of language. Are there 'things' to which sentences refer? Can sentences in two languages "mean the same thing?" Are some parts of language logically prior to others? These and similar issues constitute the topics of inquiry.

The author approaches his task with a collection of ideas. A central one can be called a 'behavioral bias.' That is to say, whatever constitutes an appropriate analysis of language, it will be based on the evidence of human dispositions to react in certain recordable ways. Now this may sound like psychology, but it is not psychology; it is a statement of the canons of evidence that are to be accepted. The comments, "Actual memories mostly are traces not of past sensations but of past conceptualizations or verbalization" (p. 3), or, "[a] . . . theory . . . is a fabric of sentences variously associated to one another and to non-verbal stimuli by the mechanism of conditioned response" (p. 11), suggest even more strongly that Ouine plans to base philosophical truth on psychological conjectures. But surely he means these comments as locutions for the kinds of behavior he plans to study,



WILLARD VAN ORMAN QUINE

not as a theory of the behavior. We find this view supported in the sentence. "Such skipping (from one statement to another far removed) which exceeds the arch analogy, seems a basically humdrum affair: a transitivity of conditioning." Thus, the psychologist who turns to the section Ways of Learning Words, with the expectation of enlightenment about verbal learning, is bound to be disappointed. Ouine does not really care (professionally) about verbal learning, but rather, say, about the fact that some words must be learned contextually, and others by analogy. He uses some of the technical jargon of S-R learning theory for exposition, not for explanation. There is good and bad in this procedure. He gives wider use to psychological coinage, yet he may undercut his own argument by an apparent dependence on psychological theory.

The first of the seven chapters, then, appears to justify the acceptance of physical things, and the social foundations of language on psychological grounds. This is not, however, the case, for the psychology is incidental to the arguments. On the other hand, the second chapter, although apparently also based on psychological phenomena, deals analytically with a problem of



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fundamental concern to the psychologist. Quine treats in this section the problem of the unambiguous interpretation of behavior that is systematically not under the control of well-defined outcome (reinforcement) schedules. He casts the problem as one of "radical translation," like learning the language of a primitive tribe. It is the same problem that the clinical psychologist faces when he attempts a case report. that the psychophysicist resolves by fiat when he scales stimuli by direct estimation techniques, that the social psychologist sidesteps when he divines attitudes from behavior.

The point is that in all these cases one interprets certain actions, verbal or otherwise, as revelatory of the state of mind of the actor. Insofar as reliable data are obtained, and correlations with other actions are conjectured and then observed, one feels secure. The problem, as Quine points out, is that a serious indeterminacy riddles our scientific serenity. The source of the indeterminacy, if I interpret him rightly, is of the following kind. We are not ever satisfied with the data raw, but always impose upon it some analysis into components, usually dictated by the current theory to which we subscribe. This analysis is tested, to be sure, but only within the context of the analysis. Two quite different analytical schemes can, as Quine indicates, lead not only to alternative interpretations, but also to contradictory ones.

The only ata immune to this detente are those waere the experimenter is the teacher. Here, the scientist has advance information about the outcome of the experiment. The only question of importance is whether the behavior can be produced. If it can, then the issues of interest concern how it came to be, what its limits are, and all the other questions of experimental psychology. Knowledge of what is wanted permits the experimenter to control the outcome schedule, and then, given that the outcomes are objects of value, the analytical problem is not sticky. The appropriate units of analysis are the behavior defined by the experimenter as leading or not leading to the reinforcement. But clearly the problems that can be cast in this form do not exhaust the interesting questions. Quine points out the difficulties, gives us some insight into the philosophical issues, and then goes on to discuss areas of linguistic analysis that are rich in philosophical puzzles.

The subsequent chapters deal with problems of reference (to what does the word red refer?), identity, ambiguity, modality, dispositions, and conditionals, to mention only a few of the topics that are discussed with lucidity and a certain charm. But in this later material, references to psychological concepts decline as the more usual apparatus of the philosopher comes into play.

At the end of the book you wonder what Quine's philosophical position really is. He says he would like to be a nominalist. But he is not-he believes in 'middle-size' objects. He also believes that certain relations, i.e., dyadic, can be used to extend the things that exist to unobservables without depending on the subjunctive forms of the naive physicalists. He is an objectivist, but he reveals the need for subjectivity to characterize the 'middle-sizedness' of those things taken for granted. He argues for the testability of sentences, and shows the inherent indeterminacy of the tests. He is aware of the complexity of the philosophical enterprise and does not choose to reduce to apparent simplicity this mass of problems that characterize scientific philosophy.

#### The Play's the Thing

Erving Goffman

The Presentation of the Self in Everyday Life. Garden City, N. Y.: Doubleday, 1959. Pp. xvi + 255. \$.95.

Reviewed by DANIEL R. MILLER

The author, Goffman, is Associate Professor of Sociology at the University of California at Berkeley. He has been at Berkeley for the past three years and for three years before that was Research Associate on the Visiting Science Program in the Laboratory of Socio-Environmental Studies of the National Institute of Mental Health. He has a PhD from the University of Chicago and has taught in the Department of Social Anthropology at the University of Edinburgh. The reviewer, Daniel Miller, is Professor of Psychology at the University of Michigan and Research Associate in its Institute of Social Research. He has reviewed Georgene Seward's Psychotherapy and Culture Conflict (Ronald, 1956; CP, May 1957. 2, 138f.) and H. W. Dunham's Sociological Theory and Mental Disorder (Wayne State Univ. Press, 1959: CP. Feb. 1961, 6, 37f.).

A<sup>MONG</sup> the activities performed by social scientists, the publishing of a book on a theoretical system must be one of the more hazardous. Such publi-

cations usually fade into oblivion-the majority because they do not integrate theory in a sufficiently creative manner to provide the empirical investigator with many new insights, and a minority because their originality makes it difficult for the average reader to connect their contents with the more fashionable ways of defining problems. It is too early to estimate the ultimate evaluation of Goffman's volume, but it should certainly be read by anyone who is interested in social relationships and who does not want to miss one of the most creative, off-beat, and frustratingly incomplete volumes written in recent years by a competent social scientist.

Goffman's topic, the ways in which people present themselves to each other, is reminiscent of *The Great God Brown*, in which Eugene O'Neill has each character hold a mask to his face whenever he expresses the publically acceptable version of his inner self; in special situations and in moments of great crisis the character abandons his virtually reflexive efforts at dissimulation and uncovers his face. While remotely related

to Jung's persona, the presented self is discussed in terms of deliberate manipulation of impressions in face-to-face situations, not the internal dynamics of an individual. To explain the significance of his topic Goffman begins by stressing the importance of first impressions for social communication and interaction When two people meet, he notes, each seeks information about the other in order to know what he will expect and how he will behave. By acting in his characteristic manner each participant in the social encounter commits himself to being a particular kind of person-say an older man who merits respect because of his mature years-and implicitly makes moral demands to be treated in a manner appropriate to that kind of person.

Starting with such standard sociological observations, Goffman proceeds to discuss the advantages of manipulating the initial impressions one conveys of oneself. Only a limited number of facts can be gleaned about a person during a particular encounter. In responding to him others are usually guided by the cues provided by his styles of expression, the content of his conversation, and his social characteristics, such as his generation and his occupation. It is obviously to a man's advantage to limit the cues to those which present him in a favorable light. Other people will then think well of him and will act voluntarily in accordance with his wishes. To look his best everyone employs techniques of stagecraft.

Concerned as he is with "aspects of the theater that creep into everyday life," Goffman has analyzed the social encounter in dramaturgical language. He labels as a "performance" all of a man's activity that influences his "audience" on a given occasion. The performer's pre-established pattern of action, which he may employ on many occasions, is called his "part" or "routine." If the dramatic action is at all complex, it must be performed by a "team" which is controlled by a "director": the minister coordinates the wedding ceremony; the umpire coordinates the action at a baseball game. There is usually a division of labor among other members of the team.

Goffman also applies the language of

the stage to the places in which social encounters occur. The routine is usually perfected in a "back region" or "backstage," which is inaccessible to the audience; it is performed in a "front region." There is also a residual region, the "outside." "Outsiders" are prevented from access to the front region so that they will not see the performance, which is intended only for the audience. The wiles that women develop in backstage conversations with each other are used in the play-acting employed for the benefit of the male audience. Children are barred as "outsiders" from both types of activity.

ONE of many possible examples of Goffman's approach is provided by his thinking about the "performance team." Among the behaviors he cites to illustrate "teamwork" are the cooperative actions of husband and wife, boss and secretary, the girls at a party, and the parts of a single individual. When a stranger is present, the husband and wife express a warmth to each other or an acceptance of the guest that may be belied by the couple's inexpressiveness or hostility when they are alone; in the presence of clients the boss and secretary forego their customary joking and use of first names; at a party the girls strive collectively to convey the impression that they are precious sexual prizes and to shun the rebel who is flagrantly accessible. In his capacity as audience a person may be taken in by the routine he performs in collusive action with other participants.

Might not any group be viewed as a team? Action groups are not. Goffman points out, since they use force or bargaining power, not dramaturgical cooperation which is aimed at sustaining a particular definition of a situation. To implement its goal a team must have certain characteristics. It must be completely united once it begins to act: any disagreement in public tends to destroy the intended definition of the situation. Even when the members have had reservations about the definition. everyone pretends that he arrived independently at the unanimous position. Usually no public stands are taken until each person knows the official position. In authoritarian organizations the superordinates try to avoid any signs of disrespecting one another and to maintain a show of always being right: parents strive to act in unison in their relationships with the children; management preser a united front to the employees, the to patients, army officers to enlight men.

The book contains many other revealing observations about teams: how they dramatize their work; how they keep secret the information that is incompatible with their public images: how they engage in collusive derogation of their audiences during performances; how they use spies to ferret out the secrets of competing teams; how they maintain the loyalty of team members. All these points are illustrated by many citations from the literature on social relationships in the family, professions, commercial establishments, military organizations, industry, simple social encounters, and intrapersonal conflicts.

GOFFMAN'S volume is an important one, but it is likely to have a mixed reception, the characteristics of which are suggested by this reviewer's experience. I first read the book five years ago, when it was published by the University of Edinburgh. At the time I was excited by the author's insights into novel and significant topics which had not been studied previously. He wrote with such clarity, simplicity, and conviction that it was easy to grasp his points and to become almost as fascinated with this new field as he himself obviously was. I was therefore happy to undertake a review of the present, expanded version, which has been published as a paperback and which differs from the original primarily in the addition of illustrative material.

My enthusiasm diminished markedly during this second reading. This time I was struck by two deficiencies, the more serious of which is the author's stress on description and classification to the virtual exclusion of theory. In one chapter, for example, he remarks that a team can perform effectively only if it keeps its secrets and if its secrets are kept by others not in the audience. Then he proposes a classification of secrets. There are "dark secrets" about facts incompatible with the team's image; there are "strategic secrets" about the team's capacities or intentions; there are "inside secrets," which mark an individual as a member of the team-he is "in the know": there are "entrusted secrets," which an outsider learns only because of his special relationship with the team; there are "free secrets" which one can disclose without being discredited.

This classification may stimulate the reader to think of many specific examples of different kinds of secrets, but to what other concepts in psychology or sociology can such esoteric information be related? Other than providing an occasional hint of possible relations, Goffman has been content to spend most of his time in perfecting his definitions of categories. His more recent papers on total institutions, embarrassment, and gambling indicate that he has definite and fruitful ideas about some of the theoretical issues to which his concepts can be applied, but he has not conveyed these ideas in the present volume.

The second deficiency in the volume is more irritating but less serious than the almost complete omission of theory. By far the largest propertion of Goffman's illustrations portray struggles between wily predators and victims, most of whom are guileless. Take some examples: on his first day of work an attendant in a psychiatric ward is told not to get caught striking a patient; in the company of her sex each woman polishes her equipment in preparation for battle, but with men she mendaciously acknowledges her lowly status in order to attain her private goals; in a garage the customer is prevented from seeing the repair of his car so he cannot judge how much his fee was affected by the mechanic's mistakes; at the same time that the announcer is extolling the sponsor's product, which is appearing on the television screen, he holds his nose in a gesture of contempt

which is outside of the camera's focus and is meant only for his gleeful teammates.

Goffman also emphasizes the duplicity in self-presentation by many of his comments: that virtually every legitimate vocation and relationship entails concealed practices which are incompatible with fostered impressions; that each person checks the impressions conveyed by other people in order to avoid being led astray or to gather ammunition for purposes of exploitation.

The seamy side of social life is a legitimate object of study, but a concentration on this topic is likely to create a one-sided picture of the subject of the book, the presentation of self. The author does not consider such topics as the presentation of an adequate self on the part of an insecure person who wants to make friends, not to exploit them; the collusive deception of a dying patient by members of his family; the happy pretense and identification with the child that underlie the parents' creation of Santa Claus. Goffman quotes Cooley to the effect that we all try to show the world the best parts of ourselves, but does not elaborate on the fact that discrepancies between the actual and presented self may result from conformity to the definition of one's roles or an attempt to perform certain acts in accordance to one's ideals.

WHILE I retain these reservations, a subsequent experience has induced me to de-emphasize them in evaluating the book. On observing people at a party, reading a novel, seeing children at play, making a purchase in a store, I found myself beginning to interpret events in terms of a new perspective, the one developed by Goffman, who had sensitized

me to dramatic aspects of the social encounter and had thus enriched my appreciation of many psychological and social events. Moreover, I found little difficulty in formulating a number of relations which are implicit in Goffman's system of classification but are not specified in the book. One such set of relations is suggested by his comments on the tendency for performers to depreciate their audiences: salesgirls enjoy the opportunity to mimic their customers; pitchmen in carnivals call their public "rubes." It seems probable that the tendency toward derogation is inversely related to the performers' social status and self-esteem, and directly related to their dependence on the audience, their predilections for the defense mechanism of projection, and their difficulties in working together as a team. Performers with low status and selfesteem can shift the blame from themselves to the audience. The more the audience's good will is required, the greater will be the team's frustrations and the more it will be inclined to use the audience as a scapegoat. The more the members favor projection, the greater will be their tendency to attribute the unwelcome part of themselves to the audience. The more widespread the dissention among the performers, the greater will be the necessity for an external scapegoat.

Numerous other topics in the book suggest groups of testable hypotheses. Certainly the many observations that stimulate speculation in the reader and the insights provided by the dramaturgical perspective justify a recommendation of Goffman's volume as required reading for everyone who is concerned with the social relationship.

U

A rose with onion for its name
Might never, never smell the same—
And canny is the nose that knows
An onion that is called a rose.

-WENDELL JOHNSON

#### English Style in the Two Cultures

Thomas A. Sebeok (Ed.)

Style in Language. Cambridge, Mass.: Technology Press, Massachusetts Institute of Technology; New York: John Wiley & Sons, 1960. Pp. xviii + 470. \$9.50.

Reviewed by ROBERT H. KNAPP

The editor, Sebeok, is a Hungarian with an AB from the University of Chicago and a PhD in Oriental Languages and Civilization from Princeton. He is now Professor of Linguistics at Indiana University, chairman of the Research Center in Anthropology, Folklore and Linguistics and also of the Graduate Program in Uralic and Altaic Studies there. He is editor of the Journal of American Folklore, and has been a Guggenheim Fellow and also a Fellow at the Center for Advanced Study in the Behavioral Sciences. He edited Myth: a Symposium (Indiana Univ. Press, 1958) and is author, with Frances Ingemann, of Studies in Cheremis (Wenner-Gren Foundation, 1956). The reviewer, Knapp. is Professor of Psychology at Wesleyan University and chairman of its Department of Psychology. He has now been at Wesleyan for fifteen years. Among his many publications are, with H. B. Goodrich, The Origins of American Scientists (Univ. Chicago Press, 1953) and. with J. J. Greenbaum, The Younger American Scholar (Univ. Chicago Press, 1954). He is now working on the sociological and educational origins of the American humanistic scholar-under a Ford Foundation grant to the Council of Humanities at Princeton.

C. Snow, the British novelist and challenging and widely known essay deploring the alienation of the sciences and humanities in our time. He speaks of the gulf of misapprehension that separates "the two cultures," and urges that this be bridged. He should take some heart, I believe, in the recent publication of Style in Language which represents a patently sincere, if at times ineffectual, effort to join the two fraternities of scholars—humanistic and

scientific-as they address themselves to that illusive but admittedly vital problem of style in literature. This volume reports no less than twenty-two separate studies with introduction, critical commentary, and final evaluation. It represents the outcome of an interdisciplinary symposium initiated by the volume's editor at the University of Indiana and supported by the Social Science Research Council. The backgrounds and interests of the participants are notably varied. Four are experimental psychologists, several are linguists, a number are from the field of cultural anthropology, while the remainder are outright humanists in English literature and philosophy. Thus, the scientific wing is borne by the psychologists, the humanistic by those in literary criticism and philosophy, while the linguists and anthropologists fall somewhere in be-

Considering the varied temperament and backgrounds of the participants, it is remarkable that they seem to have heard one another with such profit and patience, and to have derived so much stimulation from each other, a fact that is quickly conveyed by examining the final critiques and the discussions that follow each paper. Still it is a far cry from the works of psychologists, in the main, to the exquisite subjectivity of such a paper as I. A. Richards', in which he discusses the evolution of a brief poem as it formed itself successively and with much trial and many errors in his consciousness.

The characteristic concern with method and quantification of John Carroll's paper on factor analytic dimensions of style and Charles Osgood's effort to analyze the qualities of suicide notes in terms of their literary components illustrate how psychologists present data

and appeal to fact; the style is direct and functional. In contrast, contributions from the literary critics are written with a sure instinct for evocative style, a deft sense of the poetic, and an appeal to sagacity and intuition rather than quantified results.

WE may ask how psychologists fared in this encounter with their humanistic brothers. In this reviewer's opinion their contributions were solid, important, but rather unexciting. Excepted from this comment is the perceptive and largely nonquantitative study by Roger Brown and Albert Gilman of the use of the second-person pronoun in modern European languages. René Wellek, in his review of the value of the meeting observes that the psychologists, with irreproachable method, demonstrated what was largely obvious, and elsewhere Fred Householder observes that the linguists and those in literary criticism spoke a common language but found the gap between themselves and the psychologists most evident. It is Roger Brown who conjectures that the humanists seem to have an unreasonable fear of quantification and statistics while the psychologists shared an abhorrence of evaluation. Possibly these attitudes account in part for their difficulties in communication.

Perhaps one difficulty that beset the Conference from the outset was the lack of a clear definition of "style." A number are proffered. Osgood presents the best definition from among the psychologists, identifying style as "an individual's deviation from norms for the situations in which he is encoding." But it is clear that the humanists at the meeting find this a very imperfect, if indeed a comprehensible, statement. For them style takes on an evaluative significance, though retaining the mark of individuality. The editor of the volume early states the difficulty of finding an adequate definition. He refers to "the subtle and illusive puzzle, the fluid and dissonant quality of style" and muses on the significance of its being subjected to scientific investigation.

Wellek, in his final summing up of the outcome of the Conference, accords it only qualified success. He seems implicitly to acknowledge the hiatus separating the psychologists and humanists. At the same time, this effort is probably of considerable significance, possibly even in the history of psychology. At the very outset of modern psychology we had a clear and expressed concern with style and with attendant esthetic problems in the work of Fechner, Stumpf, and others. Yet the concern of psychology with the arts has been conspicuously thin—above all, in

America. The Division of Esthetics of the American Psychological Association at this writing is barely able to maintain a sufficient membership to warrant its continuing existence. If this Conference is a harbinger of things to come, may we not hopefully look to some fruitful re-examination of the arts by psychology? Surely a preoccupation which began so early in the history of psychology and has languished so long is deserving of renewed effort.

#### Can Science Help Education?

David G. Ryans

Characteristics of Teachers: Their Description, Comparison, and Appraisal. Washington, D. C. (1785 Massachusetts Ave., N.W.): American Council on Education, 1960. Pp. xxiv + 416. \$7.50.

Reviewed by DANIEL A. PRESCOTT

The director of this study and the author of this book, Dr. Ryans, is now Head of the Educational Developments Center for Research in System Development in the System Development Corporation at Santa Monica. Last year he was chairman of the Department of Educational Psychology at the University of Texas, and three years before that he was at the University of California in Los Angeles. He has a PhD from Minnesota, and has long been involved in the examining and evaluation of teachers. The reviewer, Dr. Prescott, is Professor and Director of Child Study at the University of Maryland, where he has been for fourteen years. He has an EdD from Harvard, where he later taught for four years. For eight years he was professor at the University of Chicago.

A PSYCHOLOGIST employed in a College of Education, and, I suspect, psychologists serving other professions, face a troubling dilemma as they engage in research activities. Traditionally dedicated to 'pure' research as the most respectable basis for status among fellow psychologists, they hesitate to identify themselves with the profession they serve by undertaking research de-

fined in terms of the operational problems faced by that profession. Characteristics of Teachers by David G. Ryans seems to me to illustrate this dilemma faced by so many of us and to demonstrate what happens when one tries at one time to pursue the two goals of doing respectable 'pure' research and of serving a profession in relation to one of its most fundamental needs.

This report describes and presents ways of assessing various personal, social, and professional characteristics of teachers as they are found in the United States. More than 6000 teachers in 1700 schools in more than 450 school systems in different parts of the United States were involved. The research was sponsored by the American Council on Education and supported by the Grant Foundation. Upwards of a decade of scientific effort went into the report. Yet the book avoids any attempt to establish the characteristics or classroom behavior of the effective teacher in contrast to the poor teacher, although teachers who have been rated "outstanding" and "poor" are compared. And this is despite the fact that the purpose of the whole research program was the establishment of a validated base for se-



DAVID G. RYANS

lecting teachers on the basis of some series of objective tests.

The work of Dr. Ryans and his assistants was unquestionably scientific. It gathered much significant information, which is reported clearly. In my opinion many good uses of the findings can be made by adequately trained educators. But the persons who use the information effectively and purposively must know what they believe about the educative process, must have convictions about the goals and purposes of education, and must accept responsibility for making choices designed to improve our schools. Dr. Ryans apparently was unwilling to go so far. It seems to me that a different manner of reporting the data could have made this volume one of the exciting, stimulating, and challenging educational books of our time. Of course it would have been controversial, too, but does one lose respectability as a scientist simply because he cares deeply about what his findings imply for a profession serving the children and youth of a nation in times like these?

One major phase of the research involved extensive and systematic classroom observation and assessment designed to identify "significant patterns" of teacher behavior. Other efforts were

focused on the development of self-report invertories relating to the individual teacher's preferences, experiences, attitudes, self-appraisals, and the like. These self-reports were then used as bases for estimating the probability of occurrence of specific patterns of classroom behavior, of educational attitudes, of verbal intelligence and of emotional stability. These estimates were arrived at by correlating the findings obtained through observations of the classroom activities of these teachers with their self-report inventory responses. Still other studies compared the characteristics of different groups of teachers: elementary and secondary school teachers, teachers of mathematics and science, teachers of English and social studies, teachers of different ages, married and unmarried teachers, and teachers who had attended different types of colleges and universities.

The researches reported describe a number of excellent technical achievements. They include the development of special techniques for the reliable observation and assessment of classroom behavior: the determination, largely through factor analysis, of some major generalized patterns of teacher behavior; the development of assessment instruments, made up of materials hypothetically related to teachers' classroom behavior and to various personal and social characteristics of teachers; the empirical derivations of scoring keys for these instruments on the basis of response criterion correlations; and comparisons in terms of group means for teachers classified on various bases of status and employment.

WITH regard to the procedures and outcomes of the study Dr. Arthur S. Adams. President of the American Council on Education, comments in the Foreword:

This is a research study in every sense of the phrase. Neither time or expense has been spared to make it as valid in method as such a study can be, and the conclusions are stated with careful accuracy. Consequently, the volume will be disappointing to those who seek a quick, superficial answer to one of the most difficult and complicated questions in education: What makes a good teacher? That disap-

pointment merely emphasizes the rigorous scholarship of Dr. Ryans and his associates.

Again, Dr. Ben Wood, who nearly thirty years ago pioneered in the effort to develop tests to be used in teacher selection, says in his Prefatory Note:

Dr. Ryans' proclivity for scientific methodology and for cautious interpretation of research findings has led him to prepare this report as a scholarly document. He has carefully refrained from going beyond his data or making unwarranted generalizations from his results. . . . Those who are looking for a quick and easy method for selecting youngsters who will make good teachers, or who want a simple device for screening teachers at the point of employment or promotion, will not find the answer to their quest in this report. For . . . the qualities of good teachers are not absolutes; they are, instead, interacting traits that vary in their merits depending upon educational philosophy, pupil characteristics, course level and other factors.

Both of these comments on Dr. Ryans' work annoy me greatly because they seem to be saying that 'scientific' research and 'scholarly' writing are not supposed to produce useful implications for the educational profession or exciting controversial findings about some current practices in social institutions like the school. This reviewer does not share these gloomy views about research and scholarly writing. Especially, he does not think that Dr. Ryans' studies contain little of worth for the puzzled school superintendent. Perhaps educational psychologists have a self-image so uncertain and insecure that they must bolster their respectability by assuming a pose of understatement and of refraining from speculating about the practical, operational implications of our findings for the profession. If so, it is time for a change of mood. Why should a psychologist with some valid and significant findings have to hold back from informing the profession he serves in simple direct words what he believes his findings imply? Is it for fear of losing respect from fellow psychologists?

DET us look for a moment at some of Dr. Ryans' findings. The Teacher Characteristics Study validated three patterns or syndromes of teacher behavior. Each of these patterns was demonstrated by concentrations of scores at certain points along seven-point scales from one extreme of a characteristic to the opposing extreme of this characteristic. Factor analysis of correlations between the scores along the various scales revealed the following factors as significant, related syndromes:

T.C.S. Pattern Xo—warm, understanding, friendly vs. aloof, egocentric, restricted teacher behavior

T.C.S. Pattern Yo—responsible, businesslike, systematic vs. evading, unplanned, slipshod teacher behavior

T.C.S. Pattern Zo—stimulating, imaginative, surgent vs. dull, routine teacher behavior

Among elementary school teachers the patterns Xo, Yo, and Zo were highly intercorrelated and each also seemed to be highly correlated with desired pupil behavior in the teacher's classes. They also were correlated highly with being rated a good teacher.

Now this finding seems to me to be highly significant, one that superintendents of schools and school boards would like to know about. Why is it necessary to report the finding in such a manner as to make it difficult for school administrators to know what our studies are showing? Is it really unscientific to report that our studies show that 'good' elementary school teachers are more warm, understanding, and friendly in their relationships with pupils than are teachers rated as poor? Why should we not say that 'good' elementary school teachers are more responsible, systematic, and businesslike in their assignments and in their working agreements with their pupils than are teachers who receive the rating of poor? Should we not stress the fact that our findings show that 'good' teachers are stimulating, imaginative, and surgent as they introduce children to new areas of learning, new ideas and concepts within a given subject-matter area or as they encourage original and creative expression from each developing child? It seems most unfortunate to me that the writers of both the Foreword and the Preface should warn the prospective readers away from expecting any findings of sureness or depth from this extensive and meticulous series of studies. The volume is too bashful.

#### Toward a Science of Clinical Inference

Samuel J. Beck

The Rorschach Experiment: Ventures in Blind Diagnosis. New York: Grune & Stratton, 1960. Pp. viii + 256. \$6.50.

Reviewed by MARTIN MAYMAN

The author, Samuel Beck, is the unofficial dean of the Rorschach specialists. His experience with the test has been continuous since 1927, only six years after Rorschach published his Psychodiagnostiks. He began his work under David Levy at the Institute for Child Guidance in New York. His first Rorschach publication appeared in 1930 and his work continued until 1937 when he published the first English text on Rorschach scoring and interpretation. After staff appointments at the Boston Psychopathic Hospital and later the Harvard Medical School, he became from 1936 to 1950 Head of the Psychology Laboratory at Michael Reese Hospital. Since then he has been associated with the Departments of Psychology and Psychiatry of the University of Chicago and Northwestern University as well as the Michael Reese Hospital. His interest in the Rorschach Test and his work in its behalf have continued unabated. The reviewer, Martin Mayman, has been Director of the Post-Doctoral Training Program in Clinical Psychology at the Menninger Foundation since 1951. He has conducted many seminars on Rorschach theory and on the nature of the process of clinical inference, including a workshop on the inference process at a Post-Doctoral Institute of the American Psychological Association in Washington in 1958. He is an associate member of the Topeka Psychoanalytic Society and a faculty member of the Topeka Institute for Psychoanalysis.

In four years Beck has published four books on the Rorschach test—a new edition of the first volume of his three-volume text; a translation of Bohm's

Rorschach text; a study of The Six Schizophrenias; and now this review volume. In this latest book, Beck shifts the focus of his observations from test findings to an examination of the inference process which leads from test findings to diagnostic conclusions. Most of the book deals with nine Rorschach protocols reported verbatim. With each, Beck tries to show what inferences can be drawn about the patient, the illness, and the probable response to treatment, using for these inferences no information other than that which is available within the Rorschach protocol itself. The diagnostic reports range from two and one-half pages to eleven pages in length, over half of them being seven or more pages long.

Beck is quick to warn the reader that the title of the book is something of a misnomer, for his "ventures in blind diagnosis" are not really "experiments." His effort here, he says, is not to prove the test's validity but to demonstrate its working processes. In accord with this intention, each test report is accompanied by a set of footnote explanations which trace the links between interpretive statements and the test data on which they were based. There are on the average 65 such explanatory annotations per report.

It is regrettable that these annotated reports are no more a demonstration of the "clinician's working processes" than the blind diagnostic "experiment" is really an experiment. Beck takes only a first step toward explicating the intricacies of the inference process in clinical case studies.

When, some years ago, Meehl suggested that the data-processing which

transforms a clinician's observations into diagnostic conclusions be turned over to machines programed for this purpose and equipped to do the job more rapidly, more efficiently, and more economically than the clinician can do, he aroused a good deal of indignation, and clinical psychology has not yet successfully risen to this challenge laid down by their clever devil's advocate, Experienced clinicians may know that there is more to the interpretive process than can be programed into an electronic computer. They know that the clinical art is a complex one, an intuitive one, and in many of its processes a preconscious one. It shares these attributes with the creative phases of every science. But, for the clinician to be able to demonstrate the discipline and the science behind his clinical art, he must first develop a method for achieving what Beck set out to do. He must develop appropriate techniques for the study of his own 'intuitive' working processes, techniques to pry into and make explicit the intricate network of inferences in order that he be able to establish convincingly the intrinsic reasonableness of some inferences (and the intrinsic arbitrariness of others). Surely, in time, clinical psychology will be able to demonstrate conclusively both the objective and the verifiable nature of its interpretive processes, but that day has not yet arrived.

BECK's book provides some of the raw data for a study of the clinical art. but does not undertake the study itself. Consider, for example, one of the fairly representative reports and its annotated explanations. A crude classification of its data-to-inference transformations yields an interesting frequency distribution. Thirty-two distinct inferences are drawn from familiar Rorschach variables-eleven based upon the patient's color responses, six upon the human movement responses, four upon the chiaroscuro responses, four upon the response locations, three upon the total productivity and fluctuations in productivity, and one each upon the form quality of responses, the achromatic color responses, the number of popular percepts, and Beck's new E-A score. Of the remaining thirty-five annotations,

#### McGRAW-HILL Books in Psychology: NEW and UPCOMING V

#### ☐ EDUCATING EMOTIONALLY DISTURBED CHILDREN

By NORRIS G. HARING, University of Kansas Medical School; and E. LAKIN PHILLIPS, National Orthopedic and Rehabilitation Hospital, Arlington, Virginia. McGraw-Hill Psychology and Human Development in Education Series. Available January, 1962. An upper division and graduate level text stressing the simplicity and practicality of a structured program for emotionally disturbed children in a school setting. It departs from traditional methods of treatment in giving more emphasis and support to teachers and parents on how they can actively help the recovery of the emotionally disturbed child. Details are given as to classroom handling of the educational and social-emotional problems of the child, and how home-school cooperation can be blended into a constructive program.

#### ☐ READINGS FOR AN INTRODUCTION TO PSYCHOLOGY

By RICHARD A. KING, University of North Carolina. Available January, 1962. A stimulating collection of supplementary readings especially prepared to coordinate with the Morgan text, INTRODUCTION TO PSYCHOLOGY, Second Edition. Each of the twenty chapters of this book contains from two to five articles with introductory material for each article. The articles have been selected for their historical importance, their ability to amplify points in the text, and their ability to provide an effective focus for discussion in the classroom.

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## RESEARCH IN PSYCHOTHERAPY

Edited by

ELI A. RUBINSTEIN AND MORRIS B. PARLOFF

Proceedings of an interdisciplinary conference, Washington, D. C., 1958

Continued demand for this provocative report, out of print for many months, has necessitated a second printing; copies are again available.

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## AMERICAN PSYCHOLOGICAL ASSOCIATION

Dept. RP2 1333 16th Street N. W. Washington 6, D. C. nine deal with inferences from response content, seven with inferences based upon the patient's expressive style, and nineteen are elaborations, cross references, or reformulations of statements already explained in one of the other forty-eight notes.

In a comprehensive analysis of the process of clinical inference, these explanations would comprise only a small part of the study. Other kinds of questions would be considered as well. What factors-what assumptions, thoughts, or impressions-influenced Beck to select this particular cluster of observations as the groundwork for his inference pyramid? Does his selection differ from that which would be made by other leading Rorschach workers? If so, why? Why do some observations become focal organizing points for the inference process and others not? Why does one put more reliance on some inferences than on others? To what extent does the report consist of only a few primary inferences and a large number of secondary inferences drawn not from one's test data but from the examiner's conceptual stereotypes and his personality theory? That is, to what extent does the report consist of deductive conclusions rather than inductive ones?

These are only a few of the questions one could ask of the inference process if one were tracing the full development of Beck's thought from primary data to ultimate inferences, questions Beck does not attempt to consider in this book. Rather, he turns in the last section of his book to his assumptions—provocative at times—regarding those personality attributes which he considers essential for successful psychotherapeutic intervention—assumptions by which he transposes a set of Rorschach inferences into a set of treatment recommendations.

A word of warning concerning the use which may be made of the new score which Beck proposes in the introductory section of this book—the E-A score (Erlebnis-Actual), which is computed by adding the sum M and sum C scores of a Rorschach protocol, that is to say, by adding the numerator and denominator of Rorschach's Experience Balance. There is no logical reason for objection

when someone wants to add two oranges to one apple to make three pieces of fruit, or even two apples to one good idea, to make a total of three personal possessions. The only question is: How useful is the product of such summations? The seven case illustrations do not show convincingly the superiority of the E-A score over a more qualitative assessment of the dilation or constriction of a Rorschach protocol. There are, moreover, many conceptual objections which can be raised to the suggestion that the E-A score measures the "psychologic animation" or "reflects the inner state of total psychologic vitality of the personality.

There is much in the book, however, to balance its limitations. Sprinkled through all of the theoretical and case discussions are keenly expressive phrases which enrich the meaning of Rorschach variables for the experienced practitioner as well as the novice. If one wants to learn what observations Beck relies upon most heavily in his Rorschach work, and what kinds of inferences he is most inclined to draw, this is a valuable sourcebook and provides a useful supplement to his basic text. Beck's courage in exposing to public view the reasoning behind his blind diagnostic test statements would be sufficiently rewarded if it should indicate no more about the process of clinical inference than that clinicians have not yet found a way to make fully explicit all of the implicit "psycho-logic" of clinical inferences. With more publications by Beck and others directed to these engrossing and important problems, clinicians may finally succeed in developing an articulate science of clinical inference.

I

It is axiomatic in science that every opportunity for generalization is an opportunity for progress. There are also dangers lurking behind every attempted generalization. In this particular case [cybernetics] the danger lies in the ease with which analogies can be drawn. Every model is, of course, an analogy. What makes a model heuristically valuable is that it is treated as a point of departure, not arrival.

-ANATOL RAPOPORT

#### The Pigeon Reveals the Man

George Caspar Homans

Social Behavior: Its Elementary Forms. New York: Harcourt, Brace, 1961. Pp. x + 404. \$5.50.

Reviewed by Theodore M. Newcomb

The author, Homans, is an industrial sociologist, Professor of Sociology at Harvard University, author of Fatigue of Workers (Reinhold, 1941) and The Human Group (Harcourt, Brace, 1950), a volume that has generated the one under review. The reviewer, Newcomb, is known to all psychologists as Professor of Sociology and Psychology at the University of Michigan. He has been interested in interpersonal behavior for thirty years now and has just published The Acquaintance Process (Holt, 1961; CP will review it shortly).

O tr of Homans (1950) by Skinner comes in 1961 this filly. Paternity is asserted in Chapter 2, titled Animal Behavior, and is never lost sight of thereafter. The social behavior of persons is to be "explained" (the author insists on this term) by "two classes of variable: the frequency with which activities are emitted, and the state of deprivation of the organism"—just as the pigeon's behavior in the dyadic situation of pigeon-psychologist is so explained.

Another line of ancestry is also claimed-and demonstrated. The empirical propositions that interest the author may, he says, "most easily be explained by two bodies of general propositions: behavioral psychology and elementary economics" (p. 12). The propositions about individual rewards, from psychology, are rephrased in dyadic terms, borrowed from the dismal science: thus "elementary social behavior . . . is an exchange of rewards" (p. 378). Other examples: to achieve high esteem, one must "provide services for others that are in short supply. . . . For high value received, men return high esteem" (p. 163); "investments, rewards, and costs should be 'in line' with one another" (p. 245); "the less his current profit from his behavior—the less, that is, the excess of value over cost—the .more apt he is to change his behavior; and he changes it so as to increase his profit" (p. 111). There is only a footnote reference, on a minor terminological point, to Thibaut and Kelley's systematic treatment (1959) of dyadic rewards and costs.

Homans aspires higher, of course, than merely to translate the language of contemporary social psychologists into a new dialect of Skinnerian economics. What he does attempt is to extend his earlier The Human Group (1950), which "did not try to explain much of anything," by now explaining why its empirical propositions (together with some added ones) "should take the form they do," The general, theoretical explanations come from reinforcement theory (exclusively, as far as I can see), and the framework for their extension to social-interactional behavior is that of elementary economics. The exclusive reliance, by a sociologist, on psychological principles as explanatory will surprise many readers, yet Homans' position is explicitly reductionist: "we hold that we need no new propositions to describe and explain the social. With social behavior nothing unique emerges to be analyzed only in its own terms. Rather, from the laws of individual behavior . . . follow the laws of social behavior when the complications of mutual reinforcement have been taken into account" (p. 30).

The high road of explanation is paralleled by the author's low-road insistence that he seeks not to test but only to illustrate his propositions by the research evidence that he cites. In view of the wealth of research data available, this undertaking constitutes no prob-



-Harvard University News Office George Caspar Homans

lem; the 40-odd studies cited—with very heavy reliance on six or eight of them—do indeed yield a selection of findings that may plausibly be interpreted in terms of reinforcement theory. It is easy to be plausible on the basis of selected evidence.

Few of us doubt, to begin with, that reinforcement processes are involved in many forms of human behavior, and so we find little excitement in illustrations of what we already assume to be truewhen the argument includes no consideration of alternative or supplementary explanations, and when interpretations are necessarily speculative. Let me illustrate. In Chapter 4. on Human Exchange, Homans presents five propositions, including these: "(2) The more often . . . a man's activity rewards the activity of another, the more often the other will emit the activity" and "(3) The more valuable to a man a unit of the activity another gives him, the more often he will emit activity rewarded by the activity of the other" (pp. 54-55).

In Chapter 5, entitled *Influence*, the author sets out to supply evidence that the propositions apply to men as well as to pigeons and presents data from six well-known studies as support for the propositions. These experiments, mainly on interpersonal influence, communication, conformity, and deviation,

he interprets in such language as this: "the stimulus situation is such that Person expects . . . to get more or less valuable reward . . . by changing his own behavior or by getting others to change theirs" (p. 111); "activities were in fact often exchanged for sentiments of social approval. . . . an expression of warm approval was presumed to be more valuable than one of indifference. and agreement with one's opinions more valuable than one of indifference, and agreement with one's opinions more valuable than disagreement" (p. 109). (The investigators, so far as I can discover, do not mention any "expression of warm approval.") It is even argued that if a subject changes his opinion. he has been rewarded either by "social approval of the group . . . purchased by agreement with it" or by the satisfactions of consonance-or, if he does not change his opinion, he has been rewarded by "the maintenance of his personal integrity. . . . The investigator does not mention this reward," the author continues, "but we cannot make sense of the results without it, or something much like it" (p. 97). Either you do or you don't, and in either case it's because you're rewarded; it must be so because it doesn't make sense otherwise.

Homans' pen is facile, and so is his mind-as amply revealed in his 1950 work, which was mainly devoted to showing the multiple interdependencies among frequency of interaction, sentiment (generally equated with "liking"). norms and activities (behavior not viewed as interactional). This reviewer learned much from that book and has often quoted it. The present work modifies this conceptual system only slightly, yet it shifts the emphasis to the deduction of specific propositions concerning elementary forms of behavior-whose characteristics are "shared by all mankind," and whose forms (judging by chapter titles) include influence, conformity, competition, esteem, interaction, justice, satisfaction, authority, and equality-from the general propositions of individual behavior. This labor of deduction often results in specific propositions that read like simple translations of general ones first stated in 'pigeonese' -e.g., "The more valuable to Person the activity he gets or expects to get from Other, the more often he emits the activity that gets him, or he expects will get him, that reward" (p. 110).

There are important and illuminating ideas in the book—like that of "distributive justice," the "general rule" of which is that "a man in an exchange relationship with another [i.e., in interaction with him] will expect that the rewards of each man will be proportional to his costs . . . and that the net rewards, or profits, to each man be proportional to his investments" (p. 75). No one has ever played so many variations, nor with more vivid effectiveness, on the theme of reciprocal rewards in human interaction—a literally indispensable theme in social psychology. But

neither illuminating ideas nor plausible accounts of behavior that are illustrated by selected bits of research findings can, in this reviewer's lexicon, do the work of "explaining." Elementary social behavior could surely do with a bit of explaining, but to my thinking it will take more than four principles ("the laws of individual behavior"), stated in ten lines (pp. 28-29), to do it. Perhaps the author's avowed intentions, simutaneously, of merely illustrating and also of explaining were incompatible in the first place. Surely the principles of reinforcement have to be included in a full acount of human social behavior: they are necessary. But even together with "the complications of mutual reinforcement" they do not appear to be sufficient.

#### Chemical Anxiety

A. Hoffer and Humphry Osmond

The Chemical Basis of Clinical Psychology. Springfield, Ill.: Charles C Thomas, 1960. Pp. xvi + 277. \$8.50.

Reviewed by John C. Pollard

Drs. Hoffer and Osmond are the authors of the book and, together with John Smythies, the originators of the hypothesis that the book explicates, develops, and defends. Hoffer is Director of Psychiatric Research in the University of Saskatchewan and its Hospital in Saskatoon. Osmond has been Physician Superintendent of the Saskatchewan Hospital in Weyburn and Director of Psychiatric Research there, but is now with foundation support going to England for a period of research and. writing and to take up his membership in the Royal College of Physicians. The reviewer, Pollard, is Assistant Professor of Psychiatry in The University of Michigan's Medical School and Associate Research Psychiatrist in the Mental Health Research Institute there. At present he is working on sensory deprivation, but he knows quite a lot about psychopharmacology and contributed a chapter to Uhr and Miller's Drugs and Behavior.

WITH the encouraging increases in the knowledge of brain physiology and neurochemistry the clinician can look with excitement but hesitation to new discoveries in these areas—excitement because of hope that his relatively limited ability to help large numbers of seriously ill patients might be increased, and hesitation because of his keen awareness of the urgent need for 'breakthroughs' that will 'revolutionize treatment' but so often prove to be disappointing.

Even if Hoffer and Osmond imply by the somewhat factual title of their book (1) that there is a chemical basis of clinical psychiatry, and that (2) this is it, they have nevertheless been diligent in gathering supporting evidence. The book begins with a series of extremely well-written chapters—essays in fact on "Research, the Scientific Approach and Methodology" and an historical background to the toxic agent or more practically the biochemical approach to the



#### Neo-Freudian Social Philosophy

MARTIN BIRNBACH

The implications of psychoanalytic theory for contemporary social and political problems are outlined in this analysis of the the writings of Erich Fromm, Karen Horney, Abram Kardiner, Harry Stack Sullivan, and Franz Alexander. In addition, there is an exposition of Harold D. Lasswell's important work on the application of the psychoanalytic viewpoint to politics. The main study is preceded by an explication of the principal elements of the Freudian system and their philosophical import, a description of the break with Freud's concepts, and a statement of Neo-Freudian positions on psychoanalytic theory. There is an extensive Bibliography, and a section of biographical sketches of the writers.

#### Perceval's Narrative

A PATIENT'S ACCOUNT OF HIS PSYCHOSIS, 1830-1832

JOHN PERCEVAL

Edited and with an Introduction by GREGORY BATESON

John Perceval wrote this autobiographical account of his three years of schizophrenic illness long before there were any psychiatric theories regarding schizophrenia. His story is therefore important from a scientific point of view because it is uninfluenced by Freudian and other ideas which have subsequently been proposed. Perceval tells of his illness and recovery with great vigor and insight, giving a surprisingly modern view of schizophrenia. He makes an attempt to explain why he became mentally ill and why his particular symptoms developed, and recounts a number of episodes which contributed to his recovery. \$6.75

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problems of mental illness. It is interesting that in these chapters the authors set a vigorous pace, felling psychoanalysis and Meyerian psychobiology with a few cryptic comments, and condemning the deplorable tendencies of the existentialist and phenomenologist psychiatrists, "the newer derivatives of psychoanalysis who become even more obscure than their predecessors." From these beginning essays they take the reader on a long meandering trip through what is an essential background for their hypothesis. One wonders seriously how this examination could somehow have been made more interesting. Perhaps some of its difficulty lay in the sense of indefatigable conviction that persisted throughout, a sense that the reader was not being taught but persuaded. Many travelers will falter on this journey. finding the recurrent need to refer back and recheck the relationships between one chemical and another too distracting. The following hypothesis, however, emerges.

The level of anxiety (it is to be accepted that all hitherto regarded functional disorders lie on a response continuum to anxiety, probably on a quantitative basis, with anxiety neurosis and schizophrenia at opposite ends of this continuum) depends upon the relative quantities of epinephrine to a dihydroxyindole metabolite probably 5-6 dihydroxy-N-Methylindole (DNMI). This is derived from adrenochrome, itself a derivative from epinephrine. According to the authors, epinephrine has the role of maintaining drive, interest, and emotion. Too little DNMI or too much epinephrine will cause anxiety.

While developing this hypothesis and demonstrating the psychological effects of the administration of adrenochrome and other epinephrine metabolites, the first series of case histories are presented. It is quite surprising that the authors, who are usually so astutely careful in their chemical measurements and critical of the frequent descriptive and anecdotal presentations in psychiatry, relied almost exclusively on selfreport and comparative observations by the subjects taking these experimental agents, making no attempt to make objective measurements of anxiety and other events in their subjects.

Depression, they suggest, exists in two forms—a response to excessive epinephrine (anxiety) or a response to too little epinephrine—anergic depression. The chemical differences of these two forms of depression are well defined as is their drug response, but unfortunately the authors do not set up the clinical differences clearly, so that the therapist is left with several questions. Since one group apparently will respond to a certain group of anti-depressant drugs and not the other, is his prescription going to be on a trial-and-



A. HOFFER

error basis? And do the mixed depressions seen clinically also occur chemically?

To test this hypothesis the authors present the Lysergic Acid 'psychosis' (LSD) as a suitable model. They go to considerable length to describe its appropriateness, and discuss the many objections to the consideration of this drug-induced psychosis as a schizophrenic model. Principally these objections arise because LSD psychosis is caused by (1) an exogenous toxin and (2) results in psychosis marked by visual hallucinatory phenomena whereas auditory hallucinations are more often described in schizophrenia. The authors state carefully what they expect of this model and on this description it is quite

apparent that the LSD psychosis is eminently suitable for their purposes.

It is quite beyond immediate comprehension why the authors have further to justify their use of this experimental condition by such observations that its being due to an exogenous toxin is irrelevant and that they seriously doubt whether the reported frequency of auditory hallucinations in schizophrenia is in fact as frequent as might appear because the investigator "can push the patient into agreeing that autocthonous ideas are voices." What nonsense! The LSD psychosis is quite obviously a toxic psychosis and looks like one, and, like many toxic psychoses, can be confused quite easily at times with schizophrenia. Recent experiments at the Mental Health Research Institute in Ann Arbor with LSD have reaffirmed the many previous observations that the LSD response is not only determined by dosage and subjects' personality, but also very much by their expectations of what will happen and the 'set' within which the experiment takes place. Nevertheless it would be extremely difficult to demonstrate the five Bleulerian characteristics of schizophrenia.

Concerning the mode of action of LSD there is some apparent contradiction, for a former statement that the adrenal medullary activity is increased but not the adrenocortical is later refuted by cited evidence that "LSD acts by potentiating epinephrine to increase ACTH activity," and yet again the issue seems to be further confused by the observation that LSD acts as its own best antagonist by depleting the brain of epinephrine.

THERE are many substances besides LSD that can cause "perceiving in an unhabitual way." In a brief section on Psychedelics the authors discuss a few of these and indicate that besides the production of psychotic-like states, there are many reports that the experiences are "astonishingly beautiful and valuable." From these reported experiences of artists, writers, scientists, and others, it is apparent that the usefulness of these drugs is believed to be considerably more than for the biochemical study of mental dysfunction alone. But



HUMPHRY OSMOND

unfortunately negative reports make dull reading, and it is not without interest that, when Arthur Koestler recently visited The University of Michigan, he took and experienced the effects of Psilocybin, a compound chemically and in its action not unrelated to Lysergic Acid, originally derived from a hallucinogenic Mexican mushroom. His experience was not psychedelic (mindmanifesting), not satori (enlightenment), and he did not, as he said quoting Aldous Huxley, procure "what Catholic theologians call gratuitous grace." Yet awhile, perhaps attention in this field should be directed more to the biochemical and pharmacological events than to the production of transcendental experience.

The synthesis and properties of adrenochrome and adrenolutin are discussed with an extensive review of the literature, and it is curious that the paper by Szara and Axlerod (1958). which is a fundamental discussion of basic issues, is not mentioned. Serious objections to many issues including the entire Taraxein hypothesis that a schizophrenogenic plasma substance exists are either omitted or too briefly mentioned to have them discredited. Somehow some discussion of John Benjamin's (1959) criticisms would appear to be not only appropriate but necessary. It is for this reason, that the many contrary findings are not totally evaluated in this study that this highly complex and elaborate hypothesis cannot be considered as established. One thing for sure, Hoffer and Osmond have, to deploy Claude Bernard's metaphor even further than they did, demonstrated that they themselves are skillful craftsmen with 'sortes des scalpels psychopharmacologiques.' This book is not a collection of 'empirical jigsaw

pieces,' but a scholarly if somewhat faltering development of an ingenious hypothesis. Their dedication of the book, "not as a guide but as a challenge," should be readily accepted, explicitly as it refers to the hypothesis and probably even more implicitly in that psychiatry has come of age and can now demand that its theories be born of similar scientific method and scholarship.

## But Why Are Texans Mentally Disturbed?

E. Gartly Jaco

The Social Epidemiology of Mental Disorders: A Psychiatric Survey of Texas. New York: Russell Sage Foundation, 1960. Pp. 228, \$3.50.

Reviewed by ROBERT N. WILSON

The author, Dr. Jaco, is co-editor of the Journal of Health and Human Behavior and editor of Patients. Physisians and Illness (Free Press, 1958). He is associated with the Cleveland Psychiatric Institute and Western Reserve University. His affiliation with Texans arises because he was previously Director of the University of Texas' Division of Medical Sociology. The reviewer, Dr. Wilson, is Associate Professor of Sociology in Vale University, His principal interests lie in medical sociology, social psychiatry, and the sociology of the arts. For more about him. see his review of von Mering and King. Remotivating the Mental Patient (Russell Sage Foundation, 1957; CP. Aug. 1958, 3, 2221.).

Garthy Jaco, long a student of social psychiatry and a leader in the burgeoning field of medical sociology, here presents a very careful description of the incidence of serious mental illness in Texas during 1951 and 1952. For a variety of reasons, notably the difficulties of case-finding and the hazards of agreeing upon diagnoses, the distribution of mental illness in a population is harder to determine with confidence than is the distribution of, for instance, a communicable disease. Yet it

seems clear on the basis of both scientific logic and the history of medicine that an informed attack on any disease requires some knowledge of the magnitude and location of that disease in social time and space. Thus Jaco chose to engage in a vigorous search for all new cases of psychosis among inhabitants of Texas during a two-year period.

His aim was twofold-one aspect explicit and the other implicit. The explicit goal, that of charting the occurrence of psychosis among the state's population, would seem to have been handily achieved. The implicit goal, that of generating ideas about the social causes of illness by using differential incidence rates as clues, is far less close to realization. It is faith to emphasize the fact that the author modestly limited himself to the first kind of goal. not pretending to 'explain' the genesis of severe mental disturbance in Texans. One can, however, sense his hunger for explanation, can empathize the feeling that such a fine-grained account of where and when illness happens should lead us somewhere in the quest for causal suggestions.

The epidemiological findings stem from an effort to trap all the first-treated cases of psychosis for the given years. The effort was directed to establishing 'true incidence' by throwing out an investigatory net which would capture both hospital and clinic patients and also persons in private psychiatric treatment. Jaco argues persuasively that the study of incidence rather than prevalence, of currently-becoming-ill persons rather than all mentally ill patients, is likely to prove most valuable in the search for socio-environmental roots of disease. He also makes a strong point in demonstrating that the inclusion of all newly occurring cases leads to substantially different findings than could be achieved through narrower focusing. such as restriction to first-admissions to state hospitals.

In a highly sophisticated and closely reasoned analysis, Jaco concludes that a series of demographic, ecological, and economic characteristics are related to psychosis in a nonrandom fashion. Some of his chief findings are that the rate of psychosis is higher for females than for males, for the old than for the young, for the Anglo-American subculture than for the Spanish-American or nonwhite subcultures, for single and divorced persons than for those in a state of double-blessedness.

The major question for the psychiatrist or social scientist then becomes, 'Where do we go from here?' How can epidemiological data be used to trace the etiology of illness? Although certain of the discoveries are provocative in delineating the conjunction of mental disorder and specified social characteristics, there is no sure route apparent for the elaboration of causal sequences. One might well question the adequacy of conventional sociological categories in the etiology of psychosis. Perhaps age and sex, occupation and education, ethnicity and area of residence are too gross as handles for grasping social reality in its relation to intrapsychic events. We may require more meaningful ways of abstracting from the existential flow, of recapitulating the web of interpersonal experience, and of joining that experience to the fact of individual disorder.

#### Blind Spots in Visual Theory

Hans-Lukas Teuber, William S. Battersby, and Morris B. Bender

Visual Field Defects after Penetrating Missile Wounds of the Brain. Cambridge, Mass.: Harvard University Press, for the Commonwealth Fund, 1960. Pp. xii + 143. \$4.75.

Reviewed by Mortimer Mishkin

This monograph reports a fifteen-year study, begun by Teuber and Bender in the U.S. Naval Hospital in San Diego and continued later by them in association with Battersby at the New York University-Bellevue Medical Center. Teuber is now the newly appointed chairman of the Department of Psychology at the Massachusetts Institute of Technology. Previously he was head of the Psychophysiological Laboratory at NYU-Bellevue, a unit primarily devoted to the study of brain injury in man. Battersby is Associate Professor of Neurology at Northwestern University's College of Medicine, but recently at NYU-Bellevue. Bender is Professor of Clinical Neurology at New York University's School of Medicine and Director of the Neurologic Service at the Mt. Sinai Hospital. The reviewer, Mishkin, is Research Psychologist at the National Institute of Mental Health. For a dozen years he has been working on the effects of cerebral lesions on the visually guided behavior of monkeys. chimpanzees, and man.

It is a neuroanatomical commonplace (particularly among psychologists) that the retina projects point-to-point on the striate area of the occipital lobes. If this principle of a perfect isomorphism between the visual world and the visual cortex can be taken to represent the view of the entrenched Right, then Teuber, Battersby, and Bender are the new apostles of the Left. Their well-documented dissent is based on a study of more than fifty

Veterans of World War II with visual field defects caused by shrapnel and bullet wounds of the brain. This study, complementing the dozens of similar investigations which appeared after World War I, is nevertheless unique in its combination of a large patient population, rigorous testing techniques, and extensive observations on visual performance in the so-called intact field.

The presentation, "half text, half pictures," moves steadily along from the least to the most questionable aspects of the localizationist doctrine. A few of the grosser topographic principles are accepted with little comment: The upper left retinal quadrants of both eyes project to the upper left striate cortex, the lower left quadrants to the lower left cortex, and so on. Other, finergrained relationships, such as those dealing with the level of the visual system implicated in particular shapes of defects (e.g., wedge-shaped defects after focal cortical damage; arc-shaped defects after focal damage to the subcortical pathway) are examined more closely, and the authors conclude with the mild rebuke that the currently favored scheme describing the subcortical pathway lacks solid histologic support and may be in need of revision. Nevertheless, a slightly modified scheme which they adopt is subject to the same reservation.

A MORE serious issue arises from the disclosure of an unexpectedly large number of patients in whom the blind area as defined for one eye does not match the blind area defined for the other. According to the theory of a strict retinal-cortical correspondence, a lesion high up the visual system should vield. in the binocular portion of the field. precisely the same shape of defect in both eyes. Occasional departures from perfect congruence are to be expected, either from inaccuracies of plotting or from lesions in anterior locations where fibers from corresponding retinal points have not yet become closely approximated; but the high incidence of incongruent defects found, despite careful plotting in cases with posterior injuries, requires some other explanation.

The one offered by Teuber, Battersby, and Bender is not designed to please lo-

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by Beatrice A. Wright

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calizationists. Since nearly all the binocular disparities were expressed as smaller defects in the eye opposite the damaged hemisphere, and, since the eye opposite a hemisphere sends more fibers to it than does the ipsilateral eye (in the ratio of roughly 3 to 2), the authors suggest that the smaller defect for the contralateral eye is due to the low vulnerability of its large crossed projection. This argument neglects the fact that the crossed projection is large because it includes, presumably as a partially separate bundle, all the fibers representing that eye's purely monocular field, or temporal crescent: the presence of such a bundle should not affect the vulnerability of the binocular portion of the field. To be consistent, the authors' interpretation would have to deny a separate projection for the monocular crescent, thereby implying a more irregular anatomical arrangement than even they seem willing to entertain. Yet, that there is something less than a perfect alignment of corresponding points appears to be inescapable.

The difficulties for the theory of retino-cortical correspondence do not end here with unexpected features pertaining to the defective field alone. In a final chapter dealing with the spared field of vision, the authors present evidence for widespread malfunction outside even the most sharply defined scotoma. Thus, dark adaptation may be abnormal, critical flicker frequency may be impaired, pattern recognition may be defective; and all such visual disabilities, some subtle, some more marked, are generally found throughout the 'unaffected' field. Since it is unlikely that such diffuse impairments can be ascribed to diffuse lesions in all of their cases. the authors suggest that their findings may reflect instead a diffuse retinal representation. Still another explanation seems possible. An interdependence between all parts of the visual field may be accounted for, without abandoning the scheme of a punctate retinal projection, by resorting to an interaction at the cortical level. This horizontal interaction could depend on the integrity of certain layers within the striate cortex itself, or on the integrity of surrounding nonstriate areas. There is ample evidence showing that severe visual

deficits, apparently unrelated to particular retinal loci, may be produced by nonstriate lesions in animals, and the probability is high that any missile wound in man which damages the primary visual system would damage these secondary visual areas as well. While one may thus seek in various ways to lessen the impact of their findings, the uncomfortable feeling remains that Teuber, Battersby, and Bender have seriously challenged what had seemed an all-but-final theory of the visual projection system.

## Social Factors in the Treatment of Schizophrenia

Lawrence Appleby, Jordan M. Scher, and John Cumming (Eds.)

Chronic Schizophrenia. Glencoe, Ill.: Free Press, 1960. Pp. xvi + 368. \$6.00.

Sam C. Scher and Howard R. Davis (Eds.).

The Out-Patient Treatment of Schizophrenia: A Symposium. New York: Grune & Stratton, 1960. Pp. x + 246. \$5.75.

Reviewed by Donald D. Glad

The five authors and editors are identified in the review. The reviewer, Dr. Glad, is Director of Psychology in the Greater Kansas City Mental Health Foundation, where he directs group psychotherapy and trains psychiatric residents and psychological internes. He is author of Operational Values in Psychotherapy (Oxford Univ. Press, 1959; CP. May 1961, 6, 161–163) and he has in preparation other books on group psychotherapy, the quantification of fantasy and behavior, and a test for emotional projection.

The contemporary management of schizophrenia as examined in these volumes has more of a social than a clinical manner. These symposia generally consider pathology and treatment as a function of the nature of man in society with minimal concern for a disease process.

Chronic Schizophrenia is a scholarly presentation of an Institute held at the Osawatomie, Kansas State Hospital. in 1958 with collaboration from the Menninger Foundation and the Mental Health Foundation of Greater Kansas City. The Editors include Lawrence Appleby. Senior Staff Psychologist at Osawatomie, and John Cumming, who was Director of Research for the Greater Kansas City Mental Health Foundation at the time of this symposium. Cumming is well known for his social psychiatric analyses of mental hospital functioning. Jordon M. Scher of Northwestern University has published widely on his social psychiatric investigations and is presently sparking the existential movement through the American Ontoanalytic Association and the Journal of Existential Psychiatry.

The variety of papers makes separate examination impossible. They range from consideration of the nature of man (with schizophrenia as a special case) to some puzzling reports of investigations, one of which reaches the conclusion that empirical methods are economically and therapeutically inefficient, and another which redemonstrates the Hawthorne effect, finding that schizophrenics increase in effectiveness as investigators show special interest in them.

The papers are scholarly in form, but they are previously published conceptions, programatic proposals, or reports of vaguely conceived research.

The most general premise arises from a paper by Clancey which details a platonic architecture for social therapy through structured definition of the role of everyone (including the patient). This paper is characteristic of the social-psychiatric emphasis of the entire symposium. Like the platonic ideal, however, Clancey's Organization suggests the vivacity of Lewis Carroll's dance of life ("Turn not pale beloved snail, but come and join the dance") and gives credibility to Gordon Allport's sardonic comment in another source that "some of us model man after the pigeon; others view his potentialities as many-splendored." The existential analysis in Jordon Scher's paper, together with Gregory Bateson's recognition of the double-bind experience by schizophrenics, may keep the reader sensitive to the human mockery in Clancey's conception of the social robot.

Martin Scheerer's summary is the last chapter. This shift from "psychotherapy" to "treatment organization" should be noted with favor. It is reminiscent of Hoch's comment in the *Out-Patient Treatment of Schizophrenia* that schizophrenics have taught us all—regardless of preconceptions—to help them develop social competence.

Another consensus emphasizes strengths in the schizophrenic. Galioni refers to the "healthy" aspects of personality. Appleby suggests an "appeal to that part of the ego which can exercise control." Such emphases are consistent with the value of positive reinforcement. Scheerer contrasts the positive motivational need with a custodial orientation's reward of dependent rather than constructive behavior.

"Cure" versus "socialization" is examined for empirical plausibility. Scheerer seems unable to accept "cure" and sees the conference's consensus as a "socialization" goal. The inclusion of proximal agents in "socialization" (the patient and the psychiatric aide) makes plausible sense from the conference's point of view and is comparable to Ackerman's emphasis in the Out-Patient Treatment in Schizophrenia upon the family treatment outside of the hospital.

Here are some common concepts as summarized by Scheerer. The nature of man in society is an uncertainty prior to the uncertain nature of schizophrenia. Even if a biochemical or organic basis

for schizophrenia were demonstrated, this would be a necessary rather than a sufficient condition, since otherwise sociocultural methods would be ineffective. Scheerer prefers Bertalanffy's conception of the human organism as an open system characterized by "immanent activity," not a seeking of homeostasis but the creation of a "steady-state" which maintains its own tensions and moves toward higher levels of organization. Scheerer notes that the "steady state" is consistent with the symbollevel functioning of the human organism and relates it to Goldstein's organismic concepts, the existential model. and space-time binding capacities. Certainly the conference's concern with the symbolic aspects of being human may be integrated into Bertalanffy's concep-

Scheerer hears the participants agree that early experience with significant adults contributes to schizophrenic reactions. That this agreement is nonspecific is clear, but some basis for integration is overlooked by Scheerer; for example, the schizogenic environment is one of negative definition. That would account for Bateson's "double-bind," for Scher's lack of "self," and for other problems of ego-defect and of the distortion of reality, all by the postulation of a steady-state of negation.

This volume should be useful for graduate seminars in psychopathology and should also stimulate social-psychological research.

THE other volume. The Out-Patient vatment of Schizophrenia, is like a scientific town meeting with brief papers and extensive discussions. The editors are Samuel C. Scher, Clinical Psychologist, Hamm Memorial Psychiatric Clinic, and Howard R. Davis, Director of the Minnesota Bureau of Psychological Services. Cameron's Concluding Statement makes "no attempt . . . to summarize ... " and thus the reader is left with a welter of unfinished business. Quasiagenda may be rescued by reviewing the discussions in relation to treatment. Psychoanalysis as "a reiterative method" (Rado's term) is voted down by the psychoanalysts. Some agreement on the substitution of role-retraining arises. Arieti, for example, teaches the schizophrenic to realize his own role. Abrahams engages in "role-retraining." There appears to be a consensus that the teaching of social competence is an essential psychoanalytic procedure.

Less commonly accepted is the symbiotic attachment to mother. Abrahams notes that patients want to be fed, but refuse the profferred nurturance, while Wexler takes it for granted that "feeding" is an essential beginning and Williams includes the necessity for touch experience in developing ego boundaries. Lowinger and Gottlieb use the same concept differently since patients develop a positive, dependent feeling toward the hospital and a belief in the strong, authoritative physician.

Consensus among the participants includes dependency gratification demands, resistance to the controls demanded, and a treatment resolution in terms of the development of social competence. That the symbiotic problem and the solution by role-retraining cannot be managed simultaneously is noticed by Ackerman who proposes the same resolution as does Scheerer in Chronic Schizophrenia: that, since patients differ in level of functioning, they should be treated by methods or therapists that suit developmental needs.

The Out-Patient Treatment of Schizophrenia is an exciting forum. As a source
of exercise materials for discovering any
scientific lawfulness in consensual validity, it may have merit for graduate seminars. As a scholarly sampler
of current concern with the problem.
Chronic Schizophrenia is more compelling. Neither volume may be considered as a source of new knowledge or
as an organized presentation of the fields
sampled.

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There appears to be no reason in physical nature to prevent our planet from remaining habitable for another million years, and if man can survive in spite of the dangers produced by his own frenzies, there is no reason why he should not continue the career of triumph upon which he has so recently embarked.

-BERTRAND RUSSELL

# The Banner with the Strange Device

Gordon W. Allport

Personality and Social Encounter: Selected Essays. Boston: Beacon Press, 1960. Pp. x + 386. \$7.50.

Reviewed by O. A. OESER

Gordon Allport has been Professor of Psychology at Harvard University since 1936 and connected with Harvard off and on since he was a Freshman there thirty years earlier. No further introduction is necessary since the review is entirely about him and his contribution to psychology. The reviewer, Oeser, has been Professor of Psychology at the University of Melbourne since the Second World War. Before that he was at St. Andrews University. He has a DPhil from the University of Marburg and a PhD from Cambridge University. He admits to having been influenced by the phenomenology of E. R. Jaensch, the holism of Jan Smuts, and the schemata of Frederic Bartlett. At various times he has been a visitor in America at Harvard, Yale, Chicago, and Michigan. He is a co-author of Social Structure and Personality (Routledge and Kegan Paul, 1954-58) and with F. E. Emery of Information, Decision and Action (Melbourne and Cambridge Univ. Presses, 1958; CP, Sept. 1959, 4, 283f.).

ORDON ALLPORT is one of the best-G known and most respected personalities in American psychology, one who nevertheless for many years had a disappointingly small impact on it. Fortunately his intellectual integrity, his tenacious adherence to a broad and complex conceptual schema, and his brilliant writing, have now begun to impart some momentum to the movement away from total preoccupation with the particularities of rodentology and the closed systems of learning theorists toward a reconsideration of the universals of 'personology' and open systems. Oddly enough, at least three other Harvard individualists.—James, McDougall, and Skinner—are also coming once more to be studied and taken seriously, after years in the wilderness. Why Harvard should produce so many tough oaks that do not get uprooted by every wind of change is a pretty problem in the interaction of environment and personality. For some people, Allport says, "old wine tastes better in new bottles"; but he resolutely decants his from the old and is proud that they should have a fine crust.

This volume is difficult to review briefly. It consists of 21 reprints, covering the period 1931 to 1960. They are grouped into five sections: An Approach to Personality, Motivation and Structure in Personality, Normative Problems in Personality, Group Tensions, Perception and Social Programs. Their range is from the open system of personality theory to the problems of war and peace, from ethics to ego-psychology, from metaphysics to metapsychology.

To workers in the more arid plains of "typical variables" or who "hide among the thickets of statistics" this brief catalogue might sound aversive: too wide a range, too many not exhaustively and operationally defined variables, too much synthesis without analysis. They would be wrong: to read Allport after grubbing in a narrow shaft for facts is like drinking champagne on a roof garden after a diet of prison bread and lab coffee.

From the standpoint of theory alone, this collection does not make any radical advance on his *Personality*, a *Psychological Interpretation* (1937). The

major concepts of that work, as well as of the two collections The Nature of Personality (1950) and the one under review here, are set out with beautiful lucidity and masterly compression in Becoming (1955), a book which every graduate in psychology should master if he is at all interested in people and the products of their minds. Yet, if one reads only these more abstract works, one loses much of the flavor of Allport's style, the allusions to the rich European heritage of metaphysics, philosophy, and literature, the detailed confrontation of particulate theories (and not only American ones) about this or that aspect of the human being. It is easy to be eclectic like a magpie or a writer of introductory texts. Allport's eclecticism is more difficult to attain. Thus he shows why he rejects the "social stimulus value" or 'biosocial' or 'interactional' views of personality in favor of the 'biophysical,' and then proceeds to demonstrate that an adequate account of the data can rest content with neither but requires both.

For forty years consciousness has been taboo except among the lower orders of applied and clinical psychologists, and even these, except for a few experimentalists. have operated with the concept of an empty organism pushed from the past against present resistances into an unknown future. Against them Allport uncompromisingly erected the concept of the self-conscious, rational man, who creates his style and his future with forward-looking motivations. "The literary artist creates his account; the psychologist merely compiles his."

It might be said that all the essays in this volume are a continuous becoming of the thesis that Personality is a concept which is needed to explain how and why the human being stands in history, confronting his past while creating his future. Allport does try to form a bridge from biology and psychophysics to personality, but his standpoint is that of the historian of rational achievement and not that of the psychoneural reductionist. In this aspect, too. he stands almost alone among contemporary American psychologists. To understand a historian (or a psychologist), one must understand his point of view. Toynbee and Spengler, living in a trough of the periodic curve of civilization, reach a pessimistic conclusion about its rise and decline. Allport, in a spirit of optimism but without having to draw on the consolations of theology, tries to understand and to form concepts about personality "as a relatively enduring and unique organization," worthy of study and reflection in its own right, possibly the most important study of them all, certainly the kind of study which compels one to take some ontological position.

ANY comprehensive theory, by trying to achieve too much integration, is open to criticism in details. Although Allport emphasizes both the enduring nature of personality and its stable organization or structure, his analysis of the concepts organization and structure is most unsatisfying. If, with Parsons, he believes that cultural systems maintain the boundaries of the personal system but that the person also exercises marked constraints upon the social system, we need to be given theoretical insights into the definition and measurement of 'boundaries' and 'constraints.' Otherwise it could be thought that he believes in 'the great man' theory of history. We are told that two interacting persons must maintain their required role relations in order to fulfil the needs of both. But Allport provides no systematic treatment of 'relations': what kind, how directed, how strong, whether reflexive, symmetric, transitive, or other. Without postulates about relations it is impossible to set up a systematic analysis of structure or a synthesis of structures into organizations, or even to be precise as to what is meant by 'similarity of structures.' That is why Allport's paper on models (pp. 55-68) is so profoundly unsatisfying. Nowhere does he make it explicit what the invariants are, although he tells us much about the variances. Unlike his critics, he can distinguish between ontological and causal analogies; but his insistence that the personality of the child is qualitatively different from that of the adult is not based on a systematic analysis of change or process in the morphogenesis of personality structure. Thus the criteria for distinguishing child from adult remain descriptive and rather vague.

Such increases in precision remain for the future. Allport has done psychology a service by surveying the landscapes behind and before us. His books encourage and refresh us, while we laboriously plod at beating out the next bit of the trail. And his publisher has done a service to all students by including an index as well as a complete bibliography of his writings up to 1960.

## Werneri Honoris Causa

Bernard Kaplan and Seymour Wapner (Eds.)

Perspectives in Psychological Theory: Essays in Honor of Heinz Werner. New York: International Universities Press, 1960. Pp. 384. \$7.50.

Reviewed by Alfred L. Baldwin

Both the editors have been colleagues of Werner's at Clark University for many years, Wapner since 1948 and Kaplan since 1951. Werner himself went to Clark as G. Stanley Hall Pro-Jessor in 1947. Wapner is now Professor and Kaplan Associate Professor, but the Stanley Hall title is still reserved for Werner. Wapner works with Werner on the sensori-tonic studies of perception, Kaplan with Werner on language, symbolization, and concept-formation. In fact, Werner and Kaplan have a book on these topics in progress. The reviewer, Baldwin, is Professor and Head of the Department of Child Development and Family Relationships in Cornell University's State College of Home Economics. He has been there since 1949. He is the author of Behavior and Development in Children (Dryden, 1955; CP, Apr. 1956, 1, 108-110).

In this volume, presented to Professor Heinz Werner on his seventieth birthday, his friends have contributed a set of contributions that is peculiarly suited to his wide-ranging research in-

terests and his insistence upon the essential organic unity of diverse aspects of human behavior. While the volume is admirably adapted to its purpose, it is frankly impossible to review in any integrated fashion. The contributions range from detailed reports on empirical research to highly abstract theoretical contributions, from a careful behavioristic analysis of instinctive behavior by T. C. Schneirla to a careful but not behavioristic analysis of the experiences of inner status by Silvano Arieti.

As would be expected, most of the contributors tie their offerings to Werner's own interests either in terms of holistic psychology or the concept of development. While the authors of the chapters obviously constitute no random sample, it is interesting to see the repeated emergence of the concept of maturation and the problem of the adaptive significance of development.

These themes are entirely appropriate. Both of them have had a long period of neglect in psychology, and Werner is one of the few theorists who never relinquished his insistence upon a patterned predictable course of development describable in terms of process variables. In addition, he never lost sight of the fact that development leads to organisms that are actively adaptive as well as passively adapted to their environments.

These concepts emerge especially in the contributions of J. S. Bruner and G. S. Klein, Kurt Goldstein, N. R. F. Maier, David Rapaport, Schneirla, and H. A. Witkin. Bruner and Klein present a retrospective view of the 'new look' in perception, which, despite its early emphasis on perceptual distortion, has interpreted perception as a wide functional adaptation to the environment. They confess youthful errors but maintain that, even if brash, the new look has had healthy consequences on the field. Maier's chapter is entirely concerned with the way the organism's repertoire of actions is utilized by various selector-integrator mechanisms to produce an integrated and functional behavior pattern in a particular situation. His chapter illustrates another feature of recent theorizing on adaptive behavior, namely, a concern with the actual mechanism of adaptive behavior as contrasted to an emphasis of the mere fact that behavior is adaptive.

Werner's own developmental point of view serves as the stimulus for the contributions of Rapaport, Schneirla, and Witkin. Rapaport's discussion of the developmental features of psychoanalysis is especially stimulating and interesting. It is a valuable supplement to his recent systematic formulation of psychoanalysis in Psychological Issues and in Psychology; a Science. His scholarly analysis of Freud's writings is as impressive here as always and makes us regret once again his untimely death, Witkin's chapter on The Problem of Individuality in Development, on the other hand, is a promise of exciting future publications. His recent research has explored many of the developmental correlates of field independence and dependence, including other aspects of the child's personality and the child-rearing practices of the home.

The chapters of Gardner Murphy, Martin Scheerer and M. D. Huling, and Temara Dembo deal with very different aspects of holistic psychology. Murphy tackles the difficult task of quantifying the organismic properties of a system in terms of measurement of "elements," "relations" and "paths of influence," maintaining that holistic properties can be so described. The other two chapters are reports of empirical research that has been only sketchily described in other publications. Scheerer's and Huling's long series of careful studies of the effect of embedding cues for problem solving is reported in detail. Two interesting findings are that subjects can spontaneously recall the presence of the embedded cue without recognizing its functional value, and that detached observers of the subject's behavior discover the embedded cue much more frequently than the person actively engaged in solving the problem. Dembo reports her investigation of concrete values and their interrelationship through a "seven-wishes" technique, in which the subject ranks his values through seven wishes for "his child," not for himself.

All in all the volume contains something for everyone and is a fitting tribute to one of the world's distinguished developmental psychologists.

## A New Look at Subnormality

Herbert C. Gunzburg

Social Rehabilitation of the Subnormal. London: Baillière, Tindall and Cox, 1960 (distributed by Williams & Wilkins, Baltimore, Md.). Pp. xviii + 263. \$6.50.

Reviewed by SEYMOUR SARASON

Dr. Gunzburg is Director of Psychological Services in the Monyhull Hospital in Birmingham, England, where first he worked with Dr. C. J. C. Earl, and then succeeded him. The reviewer, who thinks that Dr. Gunzburg may very well be allowed to state his own case as to how to aid in the adjustment of subnormal persons, is Director of Clinical Training at Yale University and also Professor of Psychology there.

Let the author himself sum up the intent and purpose of this interesting book:

The writer believes that this book will be of interest not only to those who are professionally concerned with the problems of subnormality. As yet there is no full nontechnical account of the institution for subnormals, showing it to be not only a harbour for the socially unfit, but an active factor in solving a social problem. The methods developed in pursuance of this aim throw new light on a little-known field of social rehabilitation. Teachers of educationally subnormal children believe usually that their time and energy have been wasted if a former pupil is admitted to an institution for subnormals. Doctors, magistrates, probation officers, welfare workers, mental health officers and police, who inevitably come into frequent contact with subnormality, often hesitate to advocate the correct course of training and treatment because of traditional prejudice. Nurses and social workers, teachers and supervisors often fail seriously to consider Mental Subnormality as a vocation and career because they believe it to be dreary, uninteresting, unrewarding and hopeless. There are indeed many people in the field of mental subnormality itself who, having been brought up in the custodial traditions, are understandably unfamiliar with the new ideas governing modern practice. These pages will, it is hoped, set into proper perspective and into the right context many of the new schemes and unusual treatment methods employed nowadays and will help to overcome much of the prejudice and resistance which are connected with the term 'mental subnormality.'

The author is a psychologist who has been associated with an English institution which for many years was headed by Dr. D. J. C. Earl who made substantive contributions to the field of mental subnormality. This book reflects the author's experience in the day-to-day operations of an institution. He describes and discusses what an institutional educational program should be like, the motive and aims of vocational training, the role of counseling, and certain special institutional training problems.

It is delightful to have the author express his opinions about important institutional issues which are too frequently skirted in the usual texts in this area. The following quotation reflects both the freshness and the boldness of the author's point of view.

A few problems have been selected for a short discussion but no attempt has been made to point to possible solutions. In many ways this chapter has been destructive in its approach because it has set out to view a few of the accepted institution practices and traditions from a slightly different angle than usual. The old system of rewards and withdrawal of privileges, it has been pointed out, has a number of disadvantages and is often inimical to our attempts at rehabilitation. Because of the subnormal's temperamental shortcomings a rigid adherence to a fair and just system of reward and punishment may make him despondent and diffident. The institution, assuming the role of the over-anxious parent ordering the life of his very young child in minute detail, can lead to the subnormal's complete inability to look after his own affairs and can make him less socially competent than he was before entering the institution.

The little discussed problems of the two sexes living under the same institutional administration have been mentioned and it has been pointed out that the sex behavior of subnormals is not more disturbing than that of other people of comparable age. A loosening of the very restrictive practices, though unsettling at first, would not cause

unsurmountable problems and could case much of the tension found in rigidly controlled institutions.

Analysis of the lives of subnormals failing in their attempts to adjust to community living, has shown again and again that these difficulties are connected with the lack of experience of communal give and take. Training for this, as well as for leisure time, are important but neglected spheres, which have never been systematically tackled.

There are precious few books concerned with institutional programs. This is one of the better attempts to stimulate students to look at our institutional practices in terms of the discrepancies which exist between practices and stated aims. One wishes it success. the stockyard worker which communicates to the reader an immediate sense of reality. The problems come through in terms of the worker's experience—issues of job and pay, racial exclusion, the narrowness of the promotional ladder, family needs, and aspirations for children. The importance of work for the individual is clearly shown, as are the satisfactions and frustrations of interpersonal relations at work—among the men, between men and foremen, and between Negro and white.

# Double Loyalty in the Stockyards

Theodore V. Purcell

Blue Collar Man: Patterns of Dual Allegiance in Industry. Cambridge, Mass.: Harvard University Press, 1960. Pp. xviii + 300. \$6,00.

Reviewed by Robert L. KAHN

The author, Purcell, is Assistant Projessor of Psychology and Industrial Relations at Loyola University in Chicago and also the director of the group that is studying human relations in the meatpacking industry there. He has an AM in economics and a Harvard PhD in psychology. The review tells more about him. The reviewer, Kahn, is Professor of Psychology at The University of Michigan and program director in the Survey Research Center there. Being primarily interested in the behavior and attitudes of people in organizational settings, he has reviewed Selznick's Leadership in Administration (Row, Peterson, 1957; CP, Jan. 1959, 4, 46) and Herzberg, Mausner, and Snyderman's The Motivation to Work (Wiley, 1959; CP. Jan. 1961, 6, 9f.).

In the controversial area of unionmanagement relations Dr. Purcell has developed a faculty almost unique among current authors: he writes books which excite the common admiration of labor leaders, company presidents, and college professors. The dust jacket of his new book includes an advertisement for his earlier exploration into the phenomenon of dual allegiance, and quotes the most unequivocal endorsements by Professors (Gordon) Allport and Slichter, union presidents Durkin and McDonald, and industrialist McCaffrey, president of the International Harvester Company. The present book comes equipped with two praising forewords by Ross Stagner and Peter Drucker, neither prone to awarding undeserved compliments.

I think I understand some of the reasons for their enthusiasm. There is a quality of authenticity about Dr. Purcell's research results. This is in part a matter of presentation; he makes extensive use of excellent verbatim responses from workers and foremen. (Such quotations make up about one third of the content of the book.) But there are deeper reasons for the authority of the data; Dr. Purcell has spent more than a decade studying packing-house workers in three cities, and his intimate knowledge of their lives gets through to the reader. Like the anthropologist, he lives in the communities he chooses for study. He was for more than two years in residence in the stockyard neighborhoods of St. Louis, Chicago, and Kansas City. At the same time, his use of stratified random sampling, a uniform basic schedule of interview questions, and nonparametric statistical analysis mark him as a competent survey technician.

From this intensive work and fusion of methods, there emerges a picture of

THE major theme of the book, as the subtitle suggests, is dual allegiance. The reader is likely to be convinced, if he was not already, that dual allegiance exists, in the sense that large numbers of workers feel simultaneous loyalty toward company and union. The existence of such a view among any considerable proportion of workers disproves those dichotomous social theories which insist on locating the lovalties of workers exclusively with companies or with unions. Moreover, the existence of dual allegiance is not only descriptive of workers; the objects of their loyalty must be sufficiently compatible to make dual allegiance tenable. Apparently, in our time and society, they are.

Allegiance can be a slippery notion, and Dr. Purcell does not make his operations for measuring it wholly clear. He does offer some definitions: "By company allegiance we mean partly a summation of the worker's companyrelated attitudes, and partly the worker's perception of 'the company as such.' By union allegiance we mean the worker's approval of the idea of having a union to represent him in the plant community." In these terms it is clear that workers accept both company and union, want both to continue as institutions, and want them to 'get along with each other.' This dual acceptance should not be too surprising. The packinghouse workers think, naturally enough, in terms of a very limited set of alternatives. The company is never discussed in relation to other kinds of employment-for example, self-employment or government service; it is thought of only in relation to other companies in the industry. Swift is compared to Armour, not some other distant reality or potentiality. In respect of the union, the basis of comparison is somewhat different. The worker cannot imagine a job without a company, but he can readily imagine a job without a union, and he doesn't want such a job. He may be critical of the union in relation to other unions or in relation to the company, but he is very clear about the fact that the union adds to his personal protection in the work situation. The workers would not describe themselves as advocates of pluralism, but they have the pragmatics of that view well in mind.

In this sense, dual allegiance is deeply rooted in the thinking of the packing-house workers. It characterizes even men who are extremely critical of both company and union in some respects—for example, the company policy of excluding non-whites from supervisory and office positions, or the readiness of some union leaders to strike before exhausting other forms of conflict resolution. Even in time of strike, the worker's acceptance of company and union as institutions seems unshaken.

The extended documentation of dual allegiance, as I have suggested, is interesting and useful. It is useful as a description of how things are; it is useful to correct the contrasting and inaccurate assertions that workers are either coerced into unions or basically hostile to management. It should also be useful as a means of improving the prediction and understanding of the workers' behavior, for example, in respect to a union decision to strike, in characteristic disregard of union exhortations at the polls, and the like.

The Blue Collar Man is, as I have already said, a quantitative comparison of three plants of the same company and industry. Such research designs are still rare; they are time-consuming and costly, but I hope that they may become more frequent. In each of the few such studies which I know, there have been important differences in the relationships discovered in one plant as compared to another. In Blue Collar, for example, Dr. Purcell finds a rank-order correlation of .90 between understanding-trust and output among workers in the Kansas City plant. He finds

the corresponding correlation in East St. Louis to be .06! What circumstances account for so large a difference we do not know. The plants differ in racial composition, in attitudes toward the work situation, and in the union which represents employees; these factors may have some bearing on the matter, and so may the combining of understanding and trust. Only through such designs as this, in which an N of organizations and situations is built up (not just an N of individuals), will we even discover the existence of such variability in relationships, let alone the cause.

For this, for his persistence in research on one important set of problems, and for his willingness to adjust his own life to the requirements of his research, Dr. Purcell's work deserves to be well received. In these respects, too, it deserves wider emulation than it has yet enjoyed.

## Psychoanalytic Pharmacology

G. J. Sarwer-Foner (Ed.)

The Dynamics of Psychiatric Drug Therapy. Springfield, Ill.: Charles C Thomas, 1960. Pp. xxviii + 624. \$16.00.

Reviewed by ROBERT E. EDWARDS

The editor, Dr. Sarwer-Foner, is Director of Psychiatric Research in Queen Mary Veterans Hospital in Montreal and Lecturer in Psychiatry at McGill University. He is a psychoanalytically trained psychiatrist, born in Poland, now a Canadian, with MDs from both McGill University and the Université de Montréal. He has published extensively on ego defenses, social psychiatry, and psychosomatic medicine. The reviewer, Dr. Edwards, is a Group Leader in the Pharmacology Section of the Sterling-Winthrop Research Institute in Rensselaer, New York. He has a PhD from the University of Washington, spent a period with Carney Landis at Psychiatric Institute and Hospital of Columbia University developing psycho-

physiological tests for human drug studies, and then taught psychology at Rensselaer Polytechnic Institute, before taking on his present post for studying the effects of drugs on animal behavior.

AT least fifteen symposia on the psy-chotropic drugs have been published within the past five years. On learning that yet another has appeared, one's first thoughts are of how it might differ, indeed how it could differ, from its predecessors. This symposium does, however, differ from the others in that the majority of the participants are psychoanalytically oriented psychiatrists, and the central problem before this group is the relation of psychoanalytic theory and practice to the use of these drugs. Of all the specialties concerned with the understanding and treatment of the behavioral disorders, the analysts have the most difficulty in reconciling their views on etiology and treatment with the psychiatric use of chemical compounds; but, in their efforts to achieve this reconciliation, they are able to contribute much to our understanding of how the pharmacological effects of drugs interact, sometimes in unexpected ways, with environmental factors and the patient's interpretation of his experiences.

This book is the edited transcript of A Conference on Psychodynamic, Psychoanalytic, and Sociologic Aspects of the Neuroleptic Drugs in Psychiatry which was held in Montreal in April of 1958. The meetings were organized so as to encourage intensive discussion of selected subjects by "balanced committees of experts," selected to insure representation of differing points of view. Five committees of six to nine men each were established and met simultaneously for three days. The reading of "working papers" by committee members was followed by lengthy group discussions which occupy nearly 40 per cent of this volume. On the third day of the conference the general conclusions of each committee were presented and discussed by all.

THE first part of the book contains the working papers and discussions of the committee on "the physiological effects of the neuroleptic drugs and their psychological implications." Such varied topics are considered as the effects of tranquilizers on locomotor and avoidance behavior in rats, human electroencephalographic and linguistic changes following medication, drug-induced catalepsy in animals, and extrapyramidal side-effects of tranquilizers in patients.

Beginning with the reports of the second committee, this book begins to diverge appreciably from the numerous published symposia on tranquilizers that have appeared since 1956. Its topic was "the effects of the neuroleptic drugs on ego defences and ego structure." N. W. Winkleman, Jr., in a paper on the use of tranquilizers during psychoanalysis, describes the primary action of these drugs as that of partially isolating the patient from disturbing and painful internal and external stimuli, thus producing a state of indifference.

Hassan Azima and R. H. Vispo report. on a five-year study of the use of anaclitic treatment and regressive electric shock during prolonged, drug-induced sleep. By these means they are able to produce profound regression in patients, as indicated by incontinence, infantile play behavior, and acceptance of bottle feeding. A case is presented in which the patient was regressed to this level within four weeks and then made a good recovery. No other data for evaluating the therapeutic merit of this procedure are given in this paper, but the behavioral effects of such an overwhelming and disorganizing method of psychophysiological brainwashing are of interest in themselves.

In a paper on psychic energetics, Mortimer Ostow suggests that "the phenothiazine tranquilizers reduce the amount of energy available to the ego by decreasing the rate of its generation in the id, and iproniazid increases the amount of energy available to the ego by facilitating the transfer of energy from id to ego." No method is given, unfortunately, for measuring psychic energy.

The third committee's topic was "the influence of the milieu and the sociological determinants of behavior." Experience with 'open hospitals' and 'therapeutic communities,' and the relation of drug therapy to milieu therapy are discussed. The committee might have been

able to delve into its subject matter in greater depth if at least one of its members were a professional sociologist or social psychologist, but this was an allpsychiatrist group.

The fourth committee discussed "transference and countertransference problems in relation to drugs." Also included in this section is an amusing, and at times disturbing, lecture by H. C. Denber on the shortcomings of scientists, as seen through the critical eyes of a psychoanalyst. His thesis is that "just as surely as a psychoanalyst reflects his own personality in the countertransference, a research worker in psychiatry reflects his inner self in the choice of subject, experimental design, analysis of data, summary and even conclusions."

The last committee considered the "therapeutic aspects of the neuroleptic drugs." Here a difference of opinion was quite evident between the psychiatrists working in large state mental institutions and those in small, private or experimental hospitals. The former tended to regard drug therapy as being of primary value, whereas the latter thought of drugs as research tools or as a means of enabling those patients who otherwise would be unapproachable to begin or continue with psychotherapy. Dr. Sarwer-Foner, the editor, presented a paper before this group in which he emphasized that the pharmacological effects of a given drug are always present if given in adequate dosage, but the therapeutic effect is dependent on many nondrug factors, such as the interpretation which the patient places on the pharmacological effects that he experiences.

Books such as this are themselves 'working papers,' awaiting the Newton of psychopharmacology who will use them in his synthesis of the field. Obviously the work of integration must be done by one man, not by a committee. Anyone still looking to committees for leadership in science might profitably read Dr. F. A. Freyhan's introductory remarks about conferences' failing to bring about synthesis (p. 98) and then turn to the final section of the book, *Conclusions*, to see how adequately the committees were able to achieve a synthesis of their working pa-

Second printing . . .

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pers. Only one of the five chairmen could present anything better than mere abstracts or summaries of the individual papers.

This book contains much of interest for one who is seriously concerned about the effects of both internal and external milieu on experience and behavior, but, as with any transcript of a conference, he must be prepared to wade through a torrent of words to find the real nuggets of insight.

## Kaleidoscope for the Depressions

Myer Mendelson

Psychoanalytic Concepts of Depression. Springfield. Ill.: Charles C. Thomas, 1960. Pp. xii + 170. S6.50.

Reviewed by ELIZABETH R. ZETZEL

The author, Dr. Mendelson, holds an appointment at the Hospital of the University of Pennsylvania. Born a Lithuanian, he has been in this hemisphere as an Intern in Victoria General Hospital in Halifax, a Resident in the Royal Victoria Hospital in Montreal, a Resident and then an Instructor at the Johns Hopkins Hospital, and then an Assistant Professor at Dalhousie University in Halifax. The reviewer, Dr. Zetzel, is Assistant Clinical Professor of Psychiatry in the Harvard Medical School, a member of the Faculty of the Boston Institute of Psychoanalysis, and a psychiatrist at the Massachusetts General Hospital. She is an American, trained during twenty years in England at London University, the London Institute of Psychoanalysis, and later at the Maudsley Hospital.

In this careful, detailed and somewhat labored monograph. Dr. Mendelson reviews psychoanalytic contributions to the subject of depression—its cause, its phenomenology, and its treatment. He attempts to place these contributions in historical perspective, to describe and compare certain key contributions, and to extrapolate broad areas of consensus

and disagreement. In conclusion, he suggests a tentative evaluation of psychoanalytic concepts of depression in the light of contemporary psychiatric and scientific understanding.

The author has tried to describe with dispassionate objectivity concepts offered by psychoanalysts of widely different orientation. It is thus inevitable that many of the reconstructions and theories which he describes have been reached by methods of investigation and treatment with which he is personally unfamiliar. This very fact, however, emphasizes the existence of certain areas of general agreement by setting them in a new and somewhat unfamiliar perspective. The similarities, for example, which apparently coexist with the widest theoretical and technical differences between such contributors as Melanie Klein and Mabel Blake Cohen illustrate the growing convergence of opinion as to the crucial significance of early object relations.

A less dramatic but equally important example concerns the long history of psychoanalytic investigation into the lowered self-esteem characteristic of depressive states, from Abraham's 1911 paper, where it was first clearly described, to Edward Bibring's important contribution of 1953. It is to be noted with regret in this context that Dr. Mendelson's review suffers from its rather cursory treatment both of this paper and of the general subject of the theory of affects. A series of important papers on this subject are dismissed with the statement: "the theory of affects is still too incomplete to throw much light on this subject at this time.' The concept of depression as a basic ego-state fundamental to contemporary psychoanalytic thought is thus underemphasized to a significant degree,

Dr. Mendelson's bibliography is impressive, and he has clearly tried to select for discussion representative contributions of different schools of thought. It should be noted, however, that he refers to no psychoanalytic papers published later than 1957. While the time involved in preparing such a monograph may explain this delay, it also points to inherent drawbacks in attempting a comprehensive and scholarly review of a topic so far in the forefront of con-

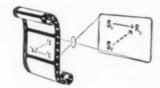
temporary scientific interest. Herbert Rosenfeld's paper of 1959, An Investigation into the Psychoanalytic Theory of Depression (Internat. J. Psychoanal., 1959, 40, 105–129), for example, covers much the same area as this monograph in a more concise and far more sophisticated manner. Also the papers presented at the XXIst International Congress of Psychoanalysis in 1959 have added significantly to the psychoanalytic literature on this subject.

Psychoanalytic discussions of depression have all too frequently made sweeping claims on insufficient evidence. Their theories, as Dr. Mendelson correctly indicates, have often been internally consistent but essentially 'closed.' The mature psychoanalyst might thus be stimulated by the very catholicity of this review to re-evaluate his own position. The partisan adherent of a particular school might object strongly to the space given to other points of view. The nonpsychoanalytic reader, finally, might well be left with the impression that deficiencies and unverifiable speculations far outweigh the positive contributions which have been made to this

To give an analogy in conclusion: the sophisticated American, aware of certain drawbacks in his own culture, would both laugh and learn from Charles Dickens' description of Martin Chuzzlewit's American adventures. Neither the prejudiced, unsophisticated American, nor his European counterpart would have the same reaction. The first might reject with anger a prejudiced critical account which he then attributed to envy. The second might eagerly accept as literal the disadvantages and drawbacks of a country toward which he looked with longing eyes. It would be easy for the polemical, narrowly informed psychoanalyst to reject this book on the grounds of lack of understanding or prejudice. It would be equally possible to accept it as an objective evaluation of psychoanalytic failure. Neither position would be any more justified than an emotional response to Dickens' caricature of America. Dr. Mendelson has attempted the impossible task of reviewing the history of an event in progress. The result is a serious though limited effort.

## INSTRUCTIONAL MEDIA

Edited by A. A. Lumsdaine



## Films and Kids: Foreign and Domestic Views

**UNESCO** 

The Influence of the Cinema on Children and Adolescents: an Annotated International Bibliography. Department of Mass Communications, UNESCO, Mass Communications Clearing House, UNESCO. Place de Fontenoy, Paris 7e, 1960. Pp. 105.

Reviewed by Eleanor E. Maccoby

The author, anonymous with respect to more specific identity, can evidently be considered as UNESCO-with whose promising start, varied subsequent fortunes, and continued high hopes mest CP readers are doubtless familiar. The reviewer, Eleanor Maccoby, is also well known to many CP readers as the principal editor, with T. M. Newcomb and E. L. Hartley, of the revised edition of Readings in Social Psychology (CP. Sept. 1959, 4, 277f.) and co-author with R. R. Sears and H. Levin of Patterns of Child Rearing (CP. Dec. 1957, 2, 305-307). She has also appeared thrice since 1956 as a reviewer in CP. Her background for this review includes her recent experience as consultant to the International Institute for Child Study in Bangkok, as well as extensive work on the effects of television and of such factors as viewer-identification with motion pictures. She is a Michigan PhD, and identifies Edwin Guthrie, with whom she earlier studied, as the most salient early influence upon her subsequent work. She is now Associate Professor of Psychology at Stanford.

This bibliography grew out of an initial listing of studies compiled by J. M. L. Peters, Director of the Netherlands Film Institute, and was

subsequently expanded by the UNESCO staff through soliciting additional entries from representatives of member states. The resulting list of 491 items, while understandably not complete, contains numerous references which American psychologists would ordinarily not encounter and so should be of considerable use to those interested in mass communications as they affect children. The time-period is given as 1930 to 1959, but the last few years of the 1950s were evidently incompletely covered. For example, the excellent Australian study of crime films (by R. I. Thomson), published in 1959, is not included. The annotation is uneven in detail; concerning the Japanese studies. little information is given except a table of contents, while for the many studies reported from Europe there is often something reported concerning methods as well as results.

The bibliography covers studies on a fairly wide variety of topics, including the problems of censorship, methods of teaching children both to appreciate and criticize "entertainment" films, and work on the production and distribution of children's films. Many studies are reported concerning the frequency of movie-going by age, sex, and social class of child, and these studies were carried

out primarily during the pre-television era. Judging by American experience. the state of affairs reported in these studies will change rapidly under the impact of television, so the studies will probably be fairly soon outdated. except in countries outside Western Europe and North America. But the studies presented under the headings. The Process of Seeing a Film and Influence and After-effects of Films are applicable to both TV-viewing and exposure to films in a motion picture theatre, and it is these studies which would probably be of the greatest interest to American psychologists. Here one finds work on children's perception of motion in films, their understanding of the time symbols used, their interpretation of the relationships between the different scenes in a film, their ability to follow a story plot, and their understanding of the motives of characters. On these topics. the work of the French psychologists. Bianka and René Zazzo, appears especially provocative. In looking over the summaries of these studies, one can occasionally find descriptions of new research techniques-e.g., asking children to use puppets to portray the plot of a movie they have just seen.

A NUMBER of researchers have dealt with the tricky topic of 'identification' with screen characters, but the annotation usually does not specify how this variable was measured; consequently it is difficult to assess the probable value of the work. One encounters the same problem in evaluating the "effect" studies: the studies for which the research methods are described range from studies in which some group (teachers, religious leaders, etc.) was polled for its opinions on the effects of movies on the moral development of children to before-and-after studies with control groups. But for many references, all that is reported is that the paper dealt with the "effects" of movies. For example, one paper is reported as "an analysis of the influence of the cinema on the physical and mental health of children" without further specification of the nature of the analysis. However, the study design is reported in a fairly large number of cases, and these reports are of value in enabling the reader to

## The Psychology of Abnormal Behavior

LOUIS P. THORPE and BARNEY KATZ, both University of Southern California; and ROBERT T. LEWIS, Los Angeles State College.

Revised by Katz and Lewis

This introductory textbook describes the nature and dynamics of abnormal behavior in simple, readable form, with terms and concepts systematically defined and clarified. Emphasis is based on the interrelationships among stress, personality, and adjustive reactions, with attention to physiological, psychological, and sociological forces. Book presents divergent views concerning the nature and operation of dynamic factors; considers normal as well as aberrant behavior patterns 94 case studies illustrate the various personality disorders and behaviors. 2nd Ed., 1961. 677 pp., illus. \$7.50

## Method in Experimental Psychology

GEORGE H. ZIMNY, Marquette University

The purpose of this textbook is to develop an understanding and an appreciation of the experimental method, especially as it is used in psychology. Several specific methods of measurement, control, and analysis are discussed in detail. Two prepared experiments including the problem, method, and set of actual data are shown as a guide in preparing reports. Book includes a wealth of helpful methodical tools. Instructor's Manual available. A volume in A Psychology Series edited by J. McV. Hunt. 1961. 366 pp., illus. \$5.50

## Intelligence and Experience

J. McV. HUNT, University of Illinois

New. Pioneering book focuses on the shift in psychological thinking from the concepts of "fixed intelligence" and "predetermined development" to an awareness of the role experience plays in the development of intelligence. It offers the first extensive review of some of Jean Piaget's later work, with illuminating quotes from his observations and experiments; reinterprets key issues in the light of new ideas and evidence. A volume in A Psychology Series edited by J. McV. Hunt. 1961. 416 pp. \$8.00

## The Meaning and Measurement of Neuroticism and Anxiety

RAYMOND B. CATTELL and IVAN H. SCHEIER —both University of Illinois

Practical handbook offers a clinically meaningful and precise description of neurosis and anxiety. It introduces mathematical models for more comprehensive diagnosis and accurate prognosis. A volume in A Psychological Series edited by J. McV. Hunt. 1961. 530 pp., illus. \$12.00

### Personality Assessment and Diagnosis

EDWARD BENNETT, formerly Tufts University

This original study describes and applies a new clinical and experimental technique for eliciting evidence of subjective feelings by means of multiple forced-choice judgments. Book applies the technique to many case histories and a research project. 1961. 287 pp., illus. \$8.00

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decide whether it would be worthwhile to try to locate and read the original publication of the study.

There exist more complete and up-todate bibliographies of the work published in English on the effects of the pictorial mass media on children (e.g., in Schramm, Lyle, and Parker, Television and Children, 1961, Stanford Press), but the UNESCO bibliography provides a much-needed source of references to material published in other languages.

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Those who desire this dreadful [psychophysical] literature can find it; it has a disciplinary value;' but I will not even enumerate it in a footnote. The only amusing part of it is that Fechner's critics should always feel bound, after smiting his theories hip and thigh and leaving not a stick of them standing, to wind up by saying that nevertheless to him belongs the imperishable glory, of first formulating them and thereby turning psychology into an exact science,

"'And everybody praised the duke
Who this great fight did win.'
'But what good came of it at last?'
Quoth little Peterkin.
'Why, that I cannot tell,' said he,
'But 'twas a famous victory!' "
—WILLIAM JAMES, 1890

Small matters! And yet James' Principles is so great a work that its lightest words carry weight; and the author is convinced that the particular words which he has been discussing have done real harm to the cause of experimental psychology in America. (To quarrel with Southey and Old Caspar is, perhaps, merely to break a butterfly on a wheel. The poem is, however, singularly fatuous, and its quotation singularly inapt.) Young students must be urged to 'plough through the difficulties' of Fechner's books, if they are presently to become psychologists: and James' criticism, which is mainly a criticism of temperament and not of reason, gives them an excuse to shirk these difficulties. Maxima debetur pueris reverentia.

-E. B. TITCHENER, 1905

### CONTEMPORARY PSYCHOLOGY

### Name Index

#### 1961

Note: Authors of Books Reviewed are always given in capital letters. Reviewers and authors of articles and letters are given in roman cap and lower case. Titles of books reviewed are in italics.

Abel, Harold, LANGFORD, 41

Abelson, H. I., "Abelson on Weiss on Abelson" (letter), 28

ABERCROMBIE, M. L. J., The anatomy of judgment (picture), 121

ABRAMSON, H. A., Neuropharmacology (5th conf.), 278e

Affleck, D. C., THORPE, 163

ALEXANDER, FRANZ, The Western mind in transition (picture), 244

Allen, D. A., "How to write the publisher" (letter), 111

Allen, R. M., KLOPFER, 236

ALLPORT, F. H., Social psychology (picture), 193

Allport, F. H., Allport, 195

Allport, G. W., Personality and social encounter, 450

Allport, G. W., "Now Allport on Wolman" (letter), 191

Amsel, Abram, Mowrer, 33

Andreas, B. G., Experimental psychology, 189

Andreas, B. G., "Is theory sine qua non?" (letter), 287

Andry, R. G., Delinquency and parental pathology, 173

Annin, Edith L., "Inside CP," 327

Apfelbaum, Bernard, "Apfelbaum on Apfelbaum" (letter), 60

Apostel, Léo, Logique, apprentissage et probabilité, 75

APPLEBY, LAWRENCE, Chronic schizophrenia, 448

ARNOLD, Magda B., Emotion and personality (picture), 289

Arnold, Magda B., "A bas la boite noire" (letter), 222

Aronson, L. J., "Sensitivity solicited" (letter), 416

Aschner, Mary Jane McC., Plaget & In-Helder, 197

ASHBY, W. R., Design for a brain (picture), 225

Atkinson, J. W., Jones, 91

AYER, A. J., Logical positivism, 88

Balley, D. E., Clinical inference and cogtive theory, 389 Bailyn, Lotte, Klapper, 394; Schramm, 158

Baker, L. M., General experimental psychology, 128

Baldwin, A. L., KAPLAN & WAPNER, 451

BALEY, STEPHAN, Wprowadzenie do psychologii spolecznej, 357

BALLOU, R. O., William James on psychical research, 149

BARRARA, D. A., Psychological and psychiatric aspects of speech and hearing, 392 BARNLUND, D. C., The dynamics of discus-

sion (picture), 208
BARITZ, LOREN, The servants of power (picture), 261

Barron, Frank, WHEELIS, 312

Bass, B. M., Leadership, psychology, and organizational behavior, 178

Bass, B. M., Barnlund & Haiman, 208 Battersby, W. S., Visual field defects after

Battersby, W. S., Visual field defects after penetrating missile wounds of the brain, 446

Battig, W. F., "For whom does the trumpet sound?" (letter), 110

BATTISTA, O. A., Mental drugs, 268 Bauer, R. A., BARITZ, 261

BEACH, F. A., The neuropsychology of Lashley, 151

Beach, F. A., "Instructional Media," 27 Beck, L. F., Eboch, 381

Beck, S. J., The Rorschach experiment, 438 Beidler, L. M., Kalmus & Hubbard, 239

Bender, M. B., Visual field defects after penetrating missile wounds of the brain, 446

Bennis, W. G., KAUFMAN, 48

Bergin, A. E., "Koch's volume 3" (letter), 415

Bergman, Paul, Lewin & Ross, 265

Berkowitz, Leonard, Cartwright & Zan-Der, 214

BERKOWITZ, PEARL H., The disturbed child,

Berlo, D. K., The process of communication, 248

BERLYNE, D. E., Conflict, arousal, and curiosity (picture), 145

Bernard, L. L., ALLPORT, 193

Bernstein, Arnold, The anatomy of psychotherapy, 82 Bettelheim, Bruno, The informed heart, 313

Bibace, Roger, Wolff, 351

Biddle, B. J., WILLNER, 363

Binder, Arnold, Ray, 281

BIRREN, J. E., Handbook of aging and the individual (picture), 113

BLAU, T. H., Private practice in clinical psychology, 74

Bligh, H. F., THOMAS, 367

BLOMMERS, P. J., Study manual, 284

BLOOD, R. O., JR., Husbands and wives, 405 Blough, D. S., SIDMAN, 279

BLUMENTHAL, J. C., English 2600, 133

Boring, E. G., "CP SPEAKS," 15, 43, 73, 127, 157, 205, 238, 267, 297, 353, 395, 428; Murphy & Ballou, 149

Boulding, K. E., Schelling, 426

Brammer, L. M., Therapeutic psychology, 269

Brazier, Mary A. B., The central nervous system and behavior (3rd conf.), 425

Bredemeier, H. C., Social problems in America, 200

Brehm, J. W., Curtis, 199

Brener, Roy, Masserman, 158

Brengelmann, J. C., Perspectives in personality research, 36

Brieland, Donald, WRIGHT, 166

BRIM, O. C., JR., Education for child rearing, 169

BROUHA, LUCIEN, Physiology in industry, 372

Brown, P. A., Haimowitz & Haimowitz, 164

Brown, Roger, Koestler, 291; "When all work is play," 329

Brožek, Josef, Birren, 113; Brouha, 372; Bujas & Petz, 316; "Recent Russian books in psychology" (list), 62; "Recent Slavic books in psychology" (list), 111

Bruner, J. S., The process of education, 356

Buck, R. C., Hanson, 257

Bugelski, B. R., An introduction to the principles of psychology, 136

BUHLER, CHARLOTTE, Der menschliche Lebenslauf als psychologisches Problem, 80 Bujas, Zoran, Osnove psihofiziologija rada, 316

Buss, A. H., The psychology of aggression (forthcoming book), 268

CAMPBELL, Angus, The American voter (picture), 202

CAMPBELL, D. T., forthcoming book. 158
Carr, W. J., "Instructional Media," 251
CARTWRIGHT, DORWIN, Group dynamics,
214

Cartwright, Rosalind D., LENNARD & BERN-STEIN, 82

Caspari, Ernst, Fuller & Thompson, 337 Chalmers, W. E., The local union-management relationship, 171

CHAPLIN, J. P., Systems and theories of psychology (picture), 417

Chin, Robert, WHITE & LIPPITT, 366

Christiansen, Bjørn, Attitudes towards foreign affairs as a function of personality, 123

CHURCH, JOSEPH, Language and the discovery of reality (forthcoming book), 43 Cleveland, S. E., "Body image and person-

ality" (letter), 28

CLOWARD, R. A., Delinquency and opportunity, 419

COHEN, JOHN, Chance, skill, and luck, 274 COLEMAN, J. C., Personality dynamics and effective behavior, 399

COLEMAN, R. P., Workingman's wife, 42 Conger, J. J., FINCH, 101

Converse, P. E., The American voter (picture), 202

Corsini, R. J., "How to improve CP" (letter), 30

Cotes, J. E., Johnson, 240

COTTLE, W. C., Procedures and preparation for counseling, 269

Cronbach, L. J., Essentials of psychological testing, 81

Crossen, Martha, "EGB and the last word," 335

Crowder, N. A., Adventures in algebra, 411; The arithmetic of computers, 25

CRUZE, W. W., Psychology in nursing, 183 Cuk, Alphonse, "More fact and less value for CP" (letter), 287

Cumings, J. N., Biochemical aspects of neurological disorders, 47

CUMMING, JOHN, Chronic schizophrenia, 448

CURTIS, J. H., Social psychology, 199

Dahlstrom, W. G., An MMPI handbook, 167

Dallenbach, K. M., "Across the years with Boring," 332

Daniel, R. S., Baker, 128

Darley, J. G., "A letter of marque," 331

David, H. P., Perspectives in personality research, 36

Davidson, K. S., Anxiety in elementary school children, 247

Davis, Hallowell, Hearing and deafness, 174

Davis, H. R., The out-patient treatment of schizophrenia, 448

Dean, S. J., POLANSKY, 362

DEMARCHE, D. F., Community resources in mental health, 306

DeMartino, M. F., Dreams and personality dynamics, 44

DEMBER, W. N., The psychology of perception (picture), 98

Dennis, Wayne, et al., "May a colleague review an author's book?" (letter), 223

Derber, Milton, The local union-management relationship, 171

DE SOLA POOL, ITHIEL, Trends in content analysis, 152

Diamond, Solomon, Information and error, 68

Dittes, J. E., "Dollard, Auld, observation, and inference" (letter), 110

Di Tullio, Benigno, Principi di criminologia clinica e psichiatria forense, 86 Doppelt, J. E., Wood, 227

Downie, N. M., Procedures and preparation for counseling, 269

Drever, James, et al., Sourcebook in psychology, 310

Dubin, William, The vocational maturity

Dubin, William, The vocational maturity of ninth-grade boys, 276

Duncan, Melba H., "The pitfalls of private practice" (letter), 254

Dunham, H. W., Sociological theory and mental disorder, 37

Durkin, Helen E., "On Ellis on Fried on Freud" (letter), 254

EBOCH, S. C., Operating audio-visual equipment, 381

Edelman, Milton, The local union-management relationship, 171

EDFELDT, A. W., Silent speech and silent reading, 220

EDWARDS, A. L., Experimental design in psychological research, 282; Workbook, 284

Edwards, R. E., SARWER-FONER, 454

EHRENTHEIL, O. F., Clinical medicine and the psychotic patient, 168

EHRMANN, WINSTON, Premarital dating behavior, 90

Eigen, L. D., CROWDER, 25

EISSLER, RUTH S., The psychoanalytic study of the child, Vol. XIV, 52

Ellis, Albert, FRIED, 38; "On Reiss and Durkin on Ellis on Fried on Freud" (letter), 382

Ericksen, S. C., CHAPLIN & KRAWIEC, 417 Ervin, Susan M., Luria & Yudovich, 20 Escalona, Sibylle, Prediction and outcome, 78

Escalona, Sibylle, "Escalona-Heider-Kagan" (letter), 253

Estes, S. G., BLAU, 74

Evans, J. L., Descriptive statistics, 104

Fearing, Franklin, DE SOLA POOL, 152

Feather, N. T., LINDZEY, 22

Featherstone, R. M., A pharmacologic approach to the study of the mind, 84 Feigl, Herbert, Ayer, 88

FELD, SHEILA, Americans view their mental health (picture), 263

Feldt, L. S., Edwards, 282

Fick, W. M., SELZNICK, 231

Fiedler, F. E., Bass, 178

Field, M. G., LEBEDINSKII, 12

Finch, S. M., Fundamentals of child psychiatry, 101

Finger, F. W., EHRMANN, 90

FISHER, MARGARET B., College education as personal development, 268

Fisher, Seymour, "Body image and personality" (letter), 28

Fishman, J. A., BERLO, 248

FITZSIMONS, RUTH M., Stuttering and personality dynamics, 392

Flagle, C. D., Operations research and systems engineering, 260

FLAVELL, J. H., The developmental psychology of Jean Piaget (forthcoming book), 397

Follettie, J. F., "Just title?" (letter), 191 Fraiberg, Louis, Psychoanalysis and American literary criticism, 10

Frank, G. H., COLEMAN, 399

Frederiksen, Norman, Cronbach, 81 Freud, Anna, The psychoanalytic study of the child, Vol. XIV, 52

FREYTAG, FREDERICKA F., The hypnoanalysis of an anxiety hysteria, 76

Fried, Edrita, The ego in love and sexuality, 38

Fuller, J. L., Behavior genetics (picture). 337

Galanter, Eugene, Evans et al., 104; Quine, 430

GALIFRET, YVES, Mechanisms of colour discrimination, 181

GARDNER, R. W., Cognitive control, 404 Garfield, S. L., WATKINS, 16

GARNER, ANN M., The mother-child interaction in psychosomatic disorders, 219

Garner, W. R., GULLIKSEN & MESSICK. 206 GHENT, LILA, Somatosensory changes after penetrating brain wounds in man, 272

GIFFIN, M. B., Psychological techniques in diagnosis and evaluation, 376

Gilbert, G. M., DAVID & BRENGELMANN, 36 Gilbert, W. M., GLAD, 161 GLAD, D. D., Operational values in psychotherapy, 161

Glad, D. D., APPLEBY et al., 448; Scher & Davis, 448

GLASER, ROBERT, Descriptive statistics, 104; Teaching machines and programmed learning (picture), 319

GOFFMAN, ERVING, The presentation of the self in everyday life, 432

Gordon, J. E., Epidemiology and mental illiess, 306

Gorlow, Leon, Berkowitz & Rothman, 11 Gottsegen, Gloria B., Professional school psychology, 102

Gottsegen, M. G., Professional school psychology, 102

Gough, H. G., Kocii, Vol. 3, 154

Grams, Armin, HARRIS, 172

Grant, C. W., SUPER et al., 276

Green, B. F., Jr., NATIONAL PHYSICAL LABORATORY, 5

GROTJAHN, MARTIN, Psychoanalysis and the family neurosis, 118

Gudeman, Jon, Origins of alcoholism, 348 Guilford, J. P., Elementary statistical exercises, 284; Intermediate statistical exercises, 284

Gulliksen, Harold, Psychological scaling, 206

Gundlach, R. H., "A boon for being first-born?" (letter), 110

Gunzburg, H. C., Social rehabilitation of the subnormal, 452

GURIN, GERALD, Americans view their mental health (picture), 263

Gustad, J. W., Brammer & Shostrom, 269; Cottle & Downie, 269

Haberstroh, C. J., MARCSON, 280

HAIMAN, F. S., The dynamics of discussion (picture), 208

HAIMOWITZ, M. L., Human development, 164

HAIMOWITZ, NATALIE R., Human development, 164

Haire, Mason, Modern organization theory, 299

HALL, C. S., Psychology, 136

HAMMOND, K. R., forthcoming book, 158
HANDEL, GERALD, Workingman's wife, 42
HANSON, N. R., Patterns of discovery (picture), 257

Hardison, O. B., Jr., FRAIBERG, 10

HARING, N. G., Discipline, achievement, and mental health, 271

HARMAN, H. H., Modern factor analysis, 374

Harrington, G. M., Hogben, 304

Harris, A. J., EDFELDT, 220

HARRIS, I. D., Normal children and mothers, 172

Harsh, C. M., ROKEACH, 170 Hartley, Ruth E., REIK, 87 HARTMANN, HEINZ, The psychoanalytic study of the child, Vol. XIV, 52

Hartup, W. W., NEILL, 347

HARVEY, O. J., Conceptual systems and personality organization (forthcoming book), 128

Haslerud, G. M., BRUNER, 356

Hastorf, A. H., Hook, 124

HAWORTH, MARY R., Projective techniques with children, 364

Heathers, Glen, THELEN, 422

HEBB, D. O., The neuropsychology of Lashley, 151

Heider, Grace M., Prediction and outcome, 78

Heine, R. W., Masserman & Moreno, 242
Hejna, R. F., Speech disorders and non-directive therapy, 392

Hemphill, J. K., Selvin, 282

Henneman, R. H., DEMBER, 98

Herrnstein, R. J., "Second thoughts on Wolman" (letter), 142

Herzberg, Frederick, The motivation to work, 9

HEYDE, MARTHA B., The vocational maturity of ninth-grade boys, 276

Hilgard, E. R., "All psychology is one," 330

HOCH, P. H., Current approaches to psychoanalysis, 198

HOCKETT, C. F., The first five minutes, 400 HOFFER, A., The chemical basis of clinical psychology (picture), 442

HOFFMAN, L. R., Automation and the worker, 94

HOFSTAETTER, P. R., Psychologie, 205

Hogben, Lancelot, Statistical theory, 304 Holland, J. G., The analysis of behavior, 378

Holland, J. G., LUMSDAINE & GLASER, 319 HOLZMAN, P. S., Cognitive control, 404

Homans, G. C., Social behavior (picture), 441

HOMME, L. E., Descriptive statistics, 104 HOOK, SIDNEY, Dimensions of mind, 124 Hsu, F. L. K., Opler, 7

Hubbard, S. J., The chemical senses in health and disease, 239

Hudgins, C. V., Davis & Silverman, 174 Huggins, W. H., Operations research and systems engineering, 260

HUNT, D. E., Conceptual systems and personality organization (forthcoming book), 128

Hunt, J. McV., Intelligence and experience (forthcoming book), 43; Motivation and experience (forthcoming book), 43

Huston, P. E., EHRENTHEIL & MARCHAND, 168

Hutt, M. L., "Hutt and Gibby on the child" (letter), 59

Hyman, H. H., CAMPBELL et al., 202

Illing, H. A., BUHLER, 80

Ingenohl, Ingo, "Should McGregor meet a payroll?" (letter), 254

INSTELLER, BARBEL, La genèse des structures logiques élémentaires, 197; The child's conception of geometry, 175

Isaacson, R. L., Bugelski, 136; Hall, 136; Williams, 136

JACO, E. G., The social epidemiology of mental disorders, 445

Janis, I. L., Psychological stress, 408

JANOWITZ, MORRIS, The professional soldier,

Jeffrey, W. E., Mowrer, 358

JELLINEK, E. M., The disease concept of alcoholism (picture), 348

Jenness, Arthur, FREYTAG, 76

JENNINGS, E. E., An anatomy of leadership, 235

Johnson, R. C., "Pollyanarchy" (letter), 191

JOHNSON, W. R., Science and medicine of

exercise and sports, 240

Joint Commission on Mental Illness

AND Health Action for mental health

AND HEALTH, Action for mental health, 293

Jonckheere, A. R., Logique, apprentissage et probabilité, 75

Jones, Eve, "Eve Jones on Shoben" (letter), 60

Jones, M. R., Nebraska symposium on mativation, 91

Kaback, Goldie R., ODLUM, 215

Kagan, Jerome, Escalona & Heider, 78
Kahn, R. L., Herzberg et al., 9; Purcell, 453

KAHN, T. C., Psychological techniques in diagnosis and evaluation, 376

Kaiser, H. F., HARMAN, 374

Kallen, D. J., Jennings, 235,

Kalmus, H., The chemical senses in health and disease, 239

KAMINSKI, GERHARD, Das Bild vom Anderen, 40

KAPLAN, BERNARD, Perspectives in psychological theory, 451

Kaplan, Bert, Passamanick, 109

KATONA, GEORGE, The powerful consumer, 216

Katona, George, "Aggregates and individuals" (letter), 322

Katz, Daniel, McGregor, 65

KAUFMAN, HERBERT, The forest ranger, 48 Kelleher, R. T., FEATHERSTONE & SIMON, 84 Kelman, H. C., Christiansen, 123

Kessen, William, "INSTRUCTIONAL MEDIA,"

Kimmel, H. D., Andreas, 189; McGuigan, 189

Kirkendall, L. A., Kronhausen & Kronhausen, 249 KLAPPER, J. T., The effects of mass communication, 394

Klein, D. C., LEIGHTON, 339

KLEIN, G. S., Cognitive control, 404

Klopfer, Bruno, LEDWITH, 298

KLOPFER, W. G., The psychological report, 236

Кларр, R. H., SEBEOK, 435

Koch, Sigmund, Psychology, Vol. 3, 154 Koella, W. P., Cummings & Kremer, 47

Koestler, Arthur, The lotus and the robot, 291

Kohrman, Robert, Motivation for child psychiatry treatment, 177

KRAWIEC, T. S., Systems and theories of psychology (picture), 417

Kremer, Michael, Biochemical aspects of neurological disorders, 47

KRIS, MARIANNE, The psychoanalytic study of the child, Vol. XIV, 52

Krković, Andjelko, Bujas & Petz, 316

Kronhausen, Eberhard, Pornography and the law, 309; Sex histories of American college men, 249

Kronhausen, Phyllis, Pornography and the law, 309; Sex histories of American college men, 249

KUENZLI, A. E., The phenomenological problem, 361

LANGFORD, LOUISE M., Guidance of the young child, 41

Lawson, Reed, Learning and behavior, 275 Lawton, M. P., "Wolman!" (letter), 223 Leavitt, H. J., Mann & Hoffman, 94

Lebidinskii, M. S., Voprosy psikhoterapii, 12

LEDWITH, NETTIE H., A Rorschach study of child development, 298

Leeper, Robert, "On resolving contrariety: author and reviewer" (letter), 414

Lehtinen, Laura E., The other child, 301 Leighton, A. H., An introduction to social psychiatry, 339

Lennard, H. L., The anatomy of psychotherapy, 82

LENNOX, MARGARET A., Epilepsy and related disorders (picture), 233

LENNOX, W. G., Epilepsy and related disorders (picture), 233

Levine, Maurice, Surgery as a human experience, 408

Levinger, George, Blood & Wolfe, 405 Levitt, Morton, Freud and Dewey on the nature of man, 228

Lewis, B. D., Psychoanalytic education in the United States (picture), 265

LEWIS, R. S., The other child, 301

LICHTENBERG, PHILIP, Motivation for child psychiatry treatment, 177

LIGHTHALL, F. F., Anxiety in elementary school children, 247

LINDQUIST, E. F., Study manual, 284

LINDZEY, GARDNER, Assessment of human motives, 22

LINTON, HARRIET B., Cognitive control, 404 LIPPITT, RONALD, Autocracy and democracy, 366

Lipset, S. M., *Political man* (picture), 317 Loevinger, Jane, forthcoming book, 158 Logan, F. A., *Incentive*, 184

LUCHINS, A. S., Rigidity of behavior (picture), 18

Luchins, A. S., "The Luchinses comment" (letter), 223

Luchins, Edith H., Rigidity of behavior (picture), 18

Luchins, Edith H., "The Luchinses comment" (letter), 223

LUMSDAINE, A. A., Teaching machines and programmed learning (picture), 319

Lumsdaine, A. A., "Instructional Media," 25, 56, 104, 133, 186, 251, 284, 319, 378, 411, 457

Lundstedt, Sven, Plunkett & Gordon, 306; Robinson et al., 306

Luria, A. R., Speech and the development of mental processes in the child, 20 Lynd, Helen M., Bettelheim, 313

Maccoby, Eleanor F., "Instructional Media," 457

Macgregor, Francis C., Social science in nursing, 183

MacGregor, Helen, Motivation for child psychiatry treatment, 177

MacLeod, R. B., Drever et al., 310

Madison, Peter, "On resolving contrariety: author and reviewer" (letter), 414 Maher, B. A., Arnold, 289

Mann, F. C., Automation and the worker,

94
MARCHLAND, W. E., Clinical medicine and

Marcson, Simon, The scientist in American industry, 280

Markle, Susan M., Blumenthal, 133 Marks, E. S., Katona, 216

the psychotic patient, 168

Martin, Grace C., Adventures in algebra, 411

Masling, Joseph, Phillips et al., 271 Massarik, Fred, Haire, 299

MASSERMAN, J. H., Progress in psychotherapy, Vol. V, 242; Science and psychoanalysis, Vol. III, 158

Massing, P. W., LIPSET, 317

MATALON, BENJAMIN, Logique, apprentissage et probabilité, 75

Matarazzo, J. D., PITTENGER et al., 400 Matarazzo, Ruth G., CRUZE, 183;  $M_{\Lambda C-}$  GREGOR, 183

Mausner, Bernard, The motivation to work, 9

Mausner, Bernard, Remitz, 125 Mayman, Martin, Beck, 438

McCord, Joan, Origins of alcoholism, 348

McCord. William, Origins of alcoholism, 348

McFann, H. H., Flagle et al., 260

McGregor, Douglas, The human side of enterprise (picture), 65

McGuigan, F. J., Experimental psychology, 189

McKee, J. P., STUART & PRUGH, 115

McKinney, Fred, FISHER & Noble, 268 MEARES, AINSLIE, Shapes of sanity, 116

Meehl, P. E., SARBIN et al., 389

Mendelson, Myer, Psychoanalytic concepts of depression, 456

Mensh, I. N., KAHN & GIFFIN, 376

Messick, Samuel, Psychological scaling, 206

Messick, Samuel, Gardner et al., 404

Metcali, J. T., McGraw-Hill encyclopedia of science and technology, 297

Meyers, C. E., Trow, 218

MICHAEL, W. B., Elementary statistical exercises, 284; Intermediate statistical exercises, 284

MILLER, D. R., Inner conflict and defense, 89

Miller, D. R., Dunham, 37; Goffman, 432 Miller, G. A., "Instructional Media," 252 Miller, J. G., Joint Commission on Mental Illness and Health, 293

Miller, W. E., The American voter (picture), 202

Mishkin, Mortimer, Teuber et al., 446

Moreno, J. L., Progress in psychotherapy, Vol. V, 242

Morgan, C. T., The neuropsychology of Lashley, 151

Morris, C. N., The vocational maturity of ninth-grade boys, 276

MOWRER, O. H., Learning theory and behavior (picture), 33; Learning theory and the symbolic processes, 358

Mucchielli, Roger, Le jeu du monde et le test du village imaginaire, 232

Murphy, A. T., Stuttering and personality dynamics, 392

Murphy, D. G., Jellinek, 348; McCord et al., 348; Ullman, 348

Murphy, Gardner, Development of the perceptual world, 423; William James psychical research, 149

Murray, D. C., "The virtues of diversity" (letter), 142

Murray, E. J., "Psychology's paperbacks" 341

Murray, Elsie, Galifret, 181

Mussen, P. H., Handbook of research methods in child development (picture), 285

NATIONAL PHYSICAL LABORATORY, Mechanization of thought processes, 5

NEILL, A. S., Summerhill, 347

Newcomb, T. M., Homans, 441

NISSEN, H. W., The neuropsychology of Lashley, 151

NOBLE, JEANNE L., College education as personal development, 268

Nordberg, R. B., "The person is not society" (letter), 383

Norton, D. W., Blommers & Lindquist, 284; Edwards, 282, 284; Guilford & Michael, 284

NUNNALLY, J. C., JR., Popular conceptions of mental health (picture), 397

Nunnally, J. C., Jr., GURIN et al., 263

O'Connell, D. N., Reiff & Scheerer, 70 Odlum, Doris M., Mental health, the nurse and the patient, 215

Oeser, O. A., Allport, 450

Oetting, E. R., Dahlstrom & Welsh, 167 Ohlin, L. E., Delinquency and opportunity, 419

O'Kelly, L. I., LENNOX & LENNOX, 233 ONQUÉ, GLORIA C., Longitudinal studies of

child personality, 354

Opter, M. K., Culture and mental health (picture), 7

Orne, M. T., Reiff & Schefrer, 70 Osmond, Humphry, The chemical basis of

clinical psychology (picture), 442 Overstreet, Phoebe L., The vocational

maturity of ninth-grade boys, 276

PASSAMANICK, BENJAMIN, Epidemiology of mental disorder, 109

Pastore, Nicholas, Wolman, 4; "Wolman's woods" (letter), 287

Patterson, C. H., "Frustration but no remedy" (letter), 415

PATTERSON, FRANKLIN, et al., The adolescent citizen, 212

Pear, T. H., The moulding of modern man (forthcoming book), 298

Perry, Natalie, Teaching the mentally retarded child, 301

PETZ, BORIS, Osnove psihofiziologija rada, 316

Pfeisser, C. C., Battista, 268

Phillips, E. L., Discipline, achievement, and mental health, 271

Phillips, Leslie, SAUL, 311

Piaget, Jean, La genèse des structures logiques élémentaires, 197; The child's conception of geometry, 175

PITTENGER, R. E., The first five minutes, 400

PLUNKETT, R. J., Epidemiology and mental illness, 306

Polansky, N. A., Social work research, 362 Pollard, J. C., Hoffer & Osmond, 442

Powell, Marvin, Gottsegen & Gottsegen, 102

Prescott, D. A., Ryans, 436

Pressey, S. L., "Instructional Media," 186 Pribram, K. H., Ashby, 225 PRUGH, D. G., The healthy child, 115 Prugh, D. G., Robertson, 132

Pubols, B. H. Jr., Logan, 184

Purcell, T. V., Blue collar man, 453 Purpura, D. P., Abramson, 278

Quine, W. Van O., Word and object (picture), 430

RABIN, A. I., Projective techniques with children, 364

Raine, Walter, "Escalona-Heider-Kagan" (letter), 253

RAINWATER, LEE, Workingman's wife, 42 RAPAPORT, DAVID, The structure of psychoanalytic theory, 118

RAY, W. S., An introduction to experimental design, 281

REIFF, ROBERT, Memory and hypnotic age regression (picture), 70

Reik, Theodor, The creation of woman, 87 Reiss, B. F., "On Ellis on Fried on Freud"

(letter), 253

Reitan, R. M., SEMMES et al., 272 REMITZ, UGO, Professional satisfaction

among Swedish bank employees, 125

Remmers, H. H., Patterson et al., 212 Restle, Frank, The psychology of judgment and choice (forthcoming book), 74

Richards, T. W., "Instructional Media," 186

RICKERS-OVSIANKINA, MARIA A., Rorschach psychology, 420

Riley, D. A., Lawson, 275

Riopelle, A. J., Brazier, 425

ROBERTSON, JAMES. Young children in hospitals, 132

Robinson, Elsa E., MILLER & SWANSON, 89 ROBINSON, REGINALD, Community resources in mental health, 306

ROKEACH, MILTON, The open and closed mind, 170

ROMMETVEIT, RAGNAR, Selectivity, intuition and halo effects in social perception. 300

Rosen, Sidney, RAINWATER et al., 42 Rosenberg, M. J., Bredemeier & Toby, 200 Ross, A. O., The practice of clinical child psychology, 39

Ross, A. O., TAYLOR, 13

Ross, Helen, Psychoanalytic education in the United States (picture), 265

ROTHMAN, ESTHER P., The disturbed child,

Rouke, F. L., CLOWARD & OHLIN, 419

Rov, R. H., Operations research and systems engineering, 260

RUBINSTEIN, S. L., Grundlagen der allgemeinen Psychologie, 406

RUEBUSH, B. K., Anxiety in elementary school children, 247

Ryans, D. G., Characteristics of teachers (picture), 436

Saltzman, I. J., Teaching Machines, Inc., 106

Sanford, F. H., forthcoming book, 127

Sanford, F. H., "A conceptual model: thalamic type," 325

SAPORTA, Sol, Readings in psycholinguistics (forthcoming book), 74

Sappenfield, B. R., Weiss, 368

Sarason, S. B., Anxiety in elementary school children, 247

Sarason, S. B., Gunzburg, 452

SARBIN, T. R., Clinical inference and cognitive theory, 389

Sarnoff, Irving, Stein et al., 180

SARWER-FONER, G. J., The dynamics of psychiatric drug therapy, 454

SAUL, L. J., Emotional maturity, 311

Scarbrough, H. E., ALEXANDER, 244

SCHACHTEL, E. G., Metamorphosis (picture), I

Schafer, Roy, Wolstein, 147

Schaie, K. W., BIRREN, 114

SCHEERER, MARTIN, Memory and hypnotic age regression (picture), 70

Scheerer, Martin. SCHACHTEL, 1

Schelling, T. C., The strategy of conflict, 426

Scher, J. M., Chronic schizophrenia, 448
Scher, S. C., The out-patient treatment of schizophrenia, 448

SCHLAIFER, ROBERT, Probability and statistics for business decisions (picture), 229

Schneider, Kurt, Clinical psychopathology,

SCHRAMM, WILBUR, Mass communications,

Schroder, H. M., Conceptual systems and personality organization (forthcoming book), 128

Scodel, Alvin, Kronhausen & Kronhausen, 309

Sears, R. R., Mussen, 385; Stone & Onqué, 354

SEBEOK, T. A., Style in language, 435

Sechrest, Lee, Schneider, 69

SELVIN, H. C., The effects of leadership, 282 SELZNICK, PHILIP, The organizational weapon, 231

SEMMES, JOSEPHINE, Somatosensory changes after penetrating brain wounds in man,

Shaffer, G. W., HOCH & ZUBIN, 198

Shaffer, L. F., Rickers-Ovsiankina, 420 Shartle, C. L., McGregor, 67

Sheffield, F. D., DIAMOND, 68

Shoben, E. J., Jr., KUENZLI, 361

SHOSTROM, E. L., Therapeutic psychology, 269

SIDMAN, MURRAY, Tactics of scientific research, 279

Siegel, Sidney, Cohen, 274

Silberman, H. F., CROWDER & MARTIN, 411; TMI-GROLIER, 411 SILVERMAN, S. R., Hearing and deafness, 174

Simmel, Marianne L., ZANGWILL, 340

SIMON, ALEXANDER, A pharmacologic approach to the study of the mind, 84

Simon, Maria D., ZELLER, 23

Singer, J. L., DEMARTINO, 44

SKINNER, B. F., The analysis of behavior, 378

STAUGHTER, STELLA S., The mentally retarded child and his parent, 301

Sloan, William, Lewis et al., 301; Perry, 301; Slaughter, 301

SMITH, F. V., Explanation of human behaviour, 46

Smith, M. B., Janowitz, 77; Nunnally, 397

Smock, C. D., Sarason et al., 247

Snyder, W. U., "A Daniel come to judgment" (letter), 58; STRUPP, 210

SNYDERMAN, BARBARA B., The motivation to work, 9

Solley, C. M., Development of the perceptual world, 423

Solley, C. M., ABERCROMBIE, 121; GARDNER et al., 404

Speisman, J. C., Stoodley, 160

SPENCE, D. P., Cognitive control, 404

Spence, K. W., Behavior theory and learning, 370

Sperber, Zanwil, Eissler et al., 52

Stagner, Ross, The local union-management relationship, 171

Stellar, Eliot, Beach et al., 151

Stengel, E., "Stengel and Cook on suicide" (letter), 60

Stephenson, William, SMITH, 46

STETLER, C. J., Descriptive statistics, 104
Stevens, S. S., "By precept and example,"

Stogdill, R. M., ANDRY, 173

328

STOKES, D. C., The American voter (picture), 202

Stolz, Lois M., BRIM, 169

Stone, A. A., Longitudinal studies of child personality, 354

STOODLEY, B. H., The concepts of Sigmund Freud, 160

STRAUSS, A. A., The other child, 301

STRUPP, H. H., Psychotherapists in action, 210

STUART, H. C., The healthy child, 115 Suchman, J. R., Piaget et al., 175

Sundberg, N. D., GARNER & WENAR, 219
SUPER, D. E., The vocational maturity of ninth-grade boys, 276

Super, D. E., MUCCHIELLI, 232

Suppes, Patrick, Apostel et al., 75

SWANSON, G. E., Inner conflict and de-

Szasz, T. S., GROTJAHN, 118; RAPAPORT, 118; WAELDER, 118

Szeminska, Alina, The child's conception of geometry, 175

TAFT, RONALD, Clinical inference and cognitive theory, 389

Tagiuri, Renato, Rommetviet, 300 Tagliacozzo, Luigi, Di Tullio, 86

Tannenbaum, A. S., Derber et al., 171
Taylor, Edith M., Psychological appraisal

of children with cerebral defects, 13
Teaching Machines, Inc., Statistical inference, 106

Teuber, H.-L., Somatosensory changes after penetrating brain wounds in man, 272; Visual field defects after penetrating missile wounds of the brain, 446

THELEN, H. A., Education and the human quest, 422

THOMAS, R. M., Judging student progress, 367

THOMPSON, W. R., Behavior genetics (picture), 337

THOMSON, ROBERT, The psychology of thinking, 246

THORPE, L. P., The psychology of mental health, 163

TIBBITTS, CLARK, Handbook of social gerontology (picture), 308

TITCHENER, J. L., Surgery as a human experience, 408

TMI-GROLIER, Fundamentals of algebra,

Tobach, Ethel, WINKELSTRAETER, 391

Toby, Jackson, Social problems in America, 200

Trow, W. C., Psychology in teaching and learning, 218

Trow, W. C., "Educational, psychological practicum" (letter), 383; "Objective vs. prejudiced disagreement" (letter), 61

ULLMAN, A. D., To know the difference, 348

Van Atta, Loche, Holland & Skinner, 378
Veroff, Joanne B., Lichtenberg et al., 177
Veroff, Joseph, Americans view their mental health (picture), 263

VIDICH, A. J., Identity and anxiety, 180 Vinacke, W. E., Luchins & Luchins, 18

WAELDER, ROBERT, Basic theory of psychoanalysis, 118

Wagle, Mildred K., Community resources in mental health, 306

Watte, R. R., Anxiety in elementary school children, 247

Waldrop, R. S., TIBBITTS, 308

Walker, E. L., BERLYNE, 145

Wallach, Lise, Solley & Murphy, 423

Wallach, M. A., Solley & Murphy, 423; Thomson, 246

WAPNER, SEYMOUR, Perspectives in psychological theory, 451

WATKINS, J. G., General psychotherapy, 16WATSON, R. I., Great psychologists (forth-coming book), 396

Weinstein, Sidney, Somatosensory changes after penetrating brain wounds in man, 272

Weiss, Edoardo, The structure and dynamics of the human mind, 368

Weiss, Irving, "Still Bellak" (letter), 30
Welsh, G. S., An MMPI handbook, 167
Wenar, Charles, The mother-child interaction in psychosomatic disorders, 219

Wepman, J. M., Barbara, 392; Hejna, 392; Murphy & Fitzsimons, 392

Wertheimer, Michael, Kaminski, 40

Wesley, Frank, Rubenstein, 406 Wheelis, Allen, The seeker, 312

WHITE, D. M., Identity and anxiety, 180 WHITE, R. K., Autocracy and democracy,

Wickens, D. D. Spence, 370

Wiener, D. N., Discipline, achievement, and mental health, 271

Wilkins, W. L., Janis, 408; Titchener & Levine, 408

WILLIAMS, G. W., Psychology, 136

WILLNER, DOROTHY, Decisions, values and groups, 363

Wilson, K. V., Schlaffer, 229

Wilson, R. N., Jaco, 445

Winkelstraeter, K. H., Das Betteln der Zoo-Tiere, 391

Wirt, R. D., Rabin & Hawortfi, 364; Ross, 39

WOLFE, D. M., Husbands and wives, 405
WOLFF, P. H., The developmental psychologies of Jean Piaget and psychoanalysis (picture), 351

Wolman, B. B., Contemporary theories and systems in psychology, 4

Wolman, B. B., Levitt, 228; "Wolman's Contemporary theories and systems" (letter), 58

WOLSTEIN, BENJAMIN, Countertransference (picture), 147

Woltmann, A. G., MEARES, 116

Wood, Dorothy A., Test construction, 227
WRIGHT, BEATRICE A., Physical disability,
166

Yntema, D. B., Hofstaetter, 205 Yudovich, F. Ia., Speech and the develop-

ment of mental processes in the child, 20

ZANDER, ALVIN, Group dynamics, 214
ZANGWILL, O. L., Cerebral dominance and

its relation to psychological function, 340 Zawadzki, Bohdan, Baley, 357

ZELLER, WILFRIED, Konstitution und Entwicklung (picture), 23

Zetzel, Elizabeth R., Mendelson, 456

ZUBIN, JOSEPH, Current approaches to psychoanalysis, 198

### Subject Index

#### 1961

Achievement, scholastic, 271f.

Acquisitiveness, in American society, 200f. Adaptation

frustration and, 395; level, 145-7

Adjustment

child (letter), 59f.; college student, 268f.; delinquent subcultures, 419f.; mental health, 163, 399f.; normal child, 172f.; nurses, 183f.; rehabilitation of disabled, 166f.

Adolescence

citizenship, 212f.; attitudes toward sex, 249f.

Affection, development of (film), 27

Affiliation, first-borns and (letter), 110f.

Aggression

foreign affairs and personality, 123f.; forthcoming book, 268

Algebra, auto-instructional programs in, 411-13

Alcoholism, 348-51

Analysis

factor, 374f; interview, 400f

Anxiety

chemistry of, 442-5; hypnotherapy, 76f.; identity and, 180f.; in elementary school children, 247f.; infant (film), 56f.; surgery and, 408-10

Aphasia, 340f

Appraisal, emotion and, 289-91

Arithmetic, computer, 25-7

Arousal

conflict, curiosity and, 145-7; reticular system, 145-7

Art

in current Western society, 244-6; therapeutic use of modeling, 116f.

Assessment

characteristics of teachers, 436f.; MMPI handbook, 167f.; motivation, 22f.; reports, 236f.

Attitude

effects of mass media, 394; foreign affairs and personality, 123f.; sex, 249f.; toward mental health, 397-9; unionmanagement, 171f.; work, 9f.; worker and automation, 94; worker, union and management, 453f.

Audiology, 174f.

Audio-visual materials

bibliography on children and cinema, 457f.; equipment, 381f. See also Films Autocracy, democracy and, 366f.

Auto-instructional materials

algebra, 411-13; analysis of behavior,

378-81; computer arithmetic, 25-7; English grammar, 133-6; machines and programing, 186 (film), 251f., 319-21; statistics, 104-9

Automation

mechanization of thought processes, 5f.; worker and, 94

Autonomy, personal, 313-15

Behavior

adaptive, 225-7, 451f; affectionate (film), 27; analysis of language, 430f.; autoinstructional program, 378-81; begging of animals, 391f.; central nervous system and, 425f.; choice, 274f.; cognitive, 404; conflict, arousal and curiosity, 145-7; consumer, 216f., 322f. (letter); effects of drugs, 84; genetics and, 337-9; learning model, 275f.; learning theory and, 33-6, 222f. (letter); micromolar theory, 184f.; motivation and drive, 91-4; organizational dynamics, 48f.; perceptual, 98f.; political, 317.f; predicting child's, 78-80, 253 (letter); premarital dating, 90f.; rigidity and set, 18f.; reinforcement and rat, 184f.; research on human, 46f.; social, 441f.; Spence's theory, 371f.; symbolic processes and, 358-61; under stress, 408-10; voting, 202-4. See also Learning Behavior deviations

alcoholism, 348-51; antisocial and criminal, 86f.; anxiety neurosis, 76f.; attempted suicide (letter), 60f.; attitudes toward, 397-9; brain-injured child, 13f., 301f.; chemistry of mental illness, 442-5; child, 39f., 52f., 60f. (letter), 101f., 177f.; constitutional factors, 69f.; delinquency, 173f.; disturbed child in classroom, 11f.; drug therapy, 268; ecology, 37f.; endocrine factors, 23f.; epidemiology, 109, 306-8, 445f.; family neurosis, 118-21; mental retardation, 301f., 452f.; MMPI assessment, 167f.; neurological disorders, 47f.; physical illness in mental patients, 168f.; police and (film), 186-9; psychosomatic disorders in children, 2196.; schizophrenia, 30 (lexer), 448f.; sexual, 38f.; speech disorders, 392f.; treatment facilities, 293-6

Belief, personality systems and, 170f.

Biblography

effects of cinema on children, 457f.; psychotherapy, 16 Books

Brunswik memorial volume, 158; forthcoming, see individual authors in NAME INDEX; journal of book reviews, 428f.; obscure publishers (letter), 111; paperbound, 341-7; reprints of psychological, 354; Russian psychology, 62f.; Slavic psychology, 111f.; Wiley publications in psychology, 15; writing textbooks, 127f.

Boring, E. G., Festschrift for, 325-35

Rrain

central nervous system and behavior, 425f.; cerebral dominance, 340f.; children with cerebral defects, 13f., 301f.; cybernetic model, 225-7; epilepsy, 233-5; injury and somatosensory functions, 272f.; neural mechanisms, 151f.; neurological disorders, 47f.; visual defects after brain wounds, 446f.

Chemistry

mental illness, 442-5; neurological disorders, 47f.; senses, 239f.

Childbirth, support during (film), 56f.

Child psychology, see Developmental psychology

Choice

guessing and gambling, 274f.; judgment and (forthcoming book), 74; vocational, 276f.

Clinical psychology

child, 39f.; cognitive theory and, 389-91; delinquency and parental pathology, 173f.; diagnosis and evaluation, 376; epidemiology of mental illness, 109, 306-8, 445f.; mental health in U. S., 263f.; MMP1 assessment, 167f.; police and disturbed persons, 186-9; private practice, 74f., 254 (letter); projective techniques with children, 365f.; Rorschach and blind diagnosis, 438f.; Rorschach and theory, 420-2; school problems, 102; speech disorders, 392f. See also Behavior deviations and Psychotherapy

Cognition

child development, 20; child's conception of geometry, 175-7; clinical inference and, 389-91; conflict and arousal, 145-7; conceptual systems and personality (forthcoming book), 128; consistencies in behavior, 404; developmental psychology of (forthcoming book), 43f.; development of logical structures, 197f.; genetic theories, 70-2; learning and sym-

bolic processes, 358-61; structured learning, 356f.

Color discrimination, 181f

Communication

children's verbal, 197f.; content analysis, 152f.; ethics of, 267; group discussion, 208f.; mass, 158, 394; nonverbal, 116f.; privileged, 297; process, 248f.; psychotherapy, 82f.; report-writing, 236f.; style in language, 435f. See also Language and Speech

Comparative psychology

begging of animals, 391f.; color vision in animals, 181f.; emotional development in monkeys, 27

Computers

arithmetic of 25-7; thinking, 5f., 124f., 252

Concepts

child's geometric, 175-7; foundations of science, 257-9; statistical, 229-31, 304f.

Conditioning

emotional responses, 33-6; stimulus-response, 145-7. See also Learning

Conflict

arousal, curiosity and, 145-7; game theory and, 426f.

Conformity

industrial society and, 244-6; organizations and, 48f.

Consciousness

phenomenology and, 361f.; unconsciousness and, 238

Constitution, Kretschmer's types, 23f. Consumer behavior, 216f., 322f. (letter)

Content

analysis, 152f.; representational and instrumental, 152f.

Control, experimental, 279f.

Counseling

classroom, 271f.; psychotherapeutic. 269–71; school psychology, 102; speech disorders, 392f.; young children, 41f.

Countertransference, 147f.

CP SPEAKS

academics as reviewers, 157f.; ad hominems, 74; aggression, 268; authors' and reviewers' biases, 73f.; automate, 353f.; Becker, 43; Buss, 268; Church, 43f; conflict of interest, 239; counter-reviewing, 73f.; CP's aspirations, 428f.; dissent, 395; Encyclopedia of science and technology, 297f.; Flavell, 396f.; great psychologists, 396; Harvey, 128; Hunt, D. E., 128; Hunt, J. McV., 43; Intelligent man's guide to science, 127; journal of book reviews, 428f.; Language for everybody, 74; Leaders in American science, 354; learning psychological German, 205; lengths of reviews, 297; Longitudinal studies of child personality, 354; Mass communications, 158; Medical fee guide, 128; memorial volume for Brunswik, 158; Mental drugs, 268; multiple refusals, 429; number of reviewers, 238f.; Pear, 298; principal author and senior author, 395f.; privileged communication, 297; Psychologie, 205; publishers of psychological books, 15; rating scales for books, 205f.; reprints in psychology, 354; Restle, 74; Saporta, 74; Schroder, 128; tolerances for inaccuracy, 267; ufufunyana, 206; Unconscious before Freud, 238; unreviewed books, 43, 353; Watson, 396; Wiley's silver anniversary, 15; women as book reviewers, 205, 395f.; writing textbooks, 127f.

Criminology

delinquent constitution, 86f.; delinquent subcultures, 419f.

Criticism

privileged, 297; psychoanalysis and literature, 10f.; social, 200f.

Culture

American, 200f.; delinquent subcultures, 419f.; Eastern philosophies and, 291f.; influence of (forthcoming book), 298; mental health and, 7f.; puberty rites, 87f.; Western society in transition, 244-6

Curiosity

conflict, arousal and, 145-7; scientific, 149-51.

Deafness, hearing and, 174f.

Decision theory, 229-31, 299f., 363f.

Deficiency, mental, see Mental retardation Delinquency

parental pathology and, 173f.; subcultures, 419f.

Democracy

autocracy and, 366f.; socioeconomic factors, 317f.

Depression, psychoanalytic concepts of, 456 Design, experimental, 279f., 281, 282f.

Developmental psychology

adaptation and environment, 451f.; adolescent citizenship, 212f.; anxiety in children, 247f.; anxiety in infant (film), 56f.; brain-injured child, 13f., 301f.; child development and adjustment (letter), 59f.; child in hospital, 56f. (film), 132; child psychiatry, 101f., 177f.; child rearing (letter), 60; child's conception of geometry, 175-7; cognition (forthcoming book), 43f.; continuity and discontinuity, 1-4; disturbed children, 11f., 39f.; emotional maturity, 311f.; guidance of young children, 41f.; handbook of aging, 113-15; handbook of research methods, 385-8; healthy child, 115f.; life cycle, 80f.; logical structures, 197f.; longitudinal studies, 354; maturation, 23f., 451f.; memory and hypnotic age regression, 70-2; mental retardation, 301f.; motivation and drive, 91-4: normal chlidren and mothers, 172f.; parent education, 169f.; Piaget and psychoanalysis, 351f.; Piaget's (forthcoming book), 396f.; predicting child's behavior (letter), 253; projective techniques with children, 365f.; psychoanalytic study of child, 52f.; psychosomatic disorders in children, 219f.; readings, 164; research methods, 78–80, 385–8; Rorschach study of children, 298f.; social class and child rearing, 89f.; Soviet, 20; stages of cognitive functioning, 70–2; Summerhill experiments, 347f.; vocational maturity, 276f.

Discipline, classroom, 271f.

Discovery, science and, 257-9

Discussion, group, 208f.

Dreams, personality and, 44

Drive

motivation and, 91-4; reduction, 145-7

anticonvulsant, 233-5; effects of, 84; mental, 268; psychiatry and, 454-6; psychotropic, 278f. See also Psychopharmacology

Dualism

consciousness and, 238; mind-body problem, 124f.

Economics

consumer behavior, 216f., 322 (letter); democracy and, 317f.; managerial, 229-31 Editorials, see CP Speaks

Education

adolescent citizenship, 212f.; audio-visual equipment, 381f.; characteristics of teachers, 436f.; for counseling, 269–71; Freud and Dewey, 228f.; mental health, 293–6, 397–9; parent, 169f.; personal development in college, 268f.; psychoanalysts', 265f., 416 (letter); psychotherapeutic, 11f., 347f.; schoolastic achievement and mental health, 271f.; school and society, 422f.; science, 356f.; Summerhill experiments, 347f. See also Auto-instructional materials

Educational psychology

achievement testing, 227f.; anxiety and learning, 247f.; evaluating student progress, 367f.; teaching and learning (letter), 383; text, 218

Einstellung, see Set

Electroencephalography, 233-5

Emotion

affection (film), 27; anxiety in infant (film), 56f.; appraisal and, 289-91; child in hospital, 56f. (film), 132; conditioned responses, 33-6; fear of surgery, 408-10; genetic implications, 1-4; hostility, 311f.; perception and, 98f.; support during childbirth (film), 56f.

Engineering

human, in Yugoslavia, 316f.; systems,

Epidemiology, of mental illness, 109, 306-8, 445f.

Epilepsy, 233-5

ESP, see Parapsychology

Esthetics

behavioral theory, 145-7; style in language, 435f.

Exercise, physiology of, 240f.

Existentialism, psychoanalysis and, 244-6 Experience

intelligence and (forthcoming book), 43; motivation and (forthcoming book), 43

Experimental psychology, texts, 128f., 189f., 287 (letter)

#### Family

psychoanalysis, 118-21; structure and function, 405

Festschriften

Boring, E. G., 325-35; Werner, Heinz,

Films

anxiety in infant, 56f.; child in hospital, 56f.; computers and thinking, 252; development of affection, 27; support during childbirth, 56f.; teaching machines, 186, 251f.

Freudian psychology

concepts, 160f.; Federn and, 369f.; Freud and Dewey, 228f.; psychoanalysis and literary criticism, 10f.; theory, 118-21

Gambling, psychology of, 274f.

Genetics

behavior and, 337-9; chemical senses, 239f.; epilepsy, 233-5

Geometry, child's conception of, 175-7 Gerontology

handbook, 113-15; neurobiology of aging, 113f.; psychological aspects of aging, 114f.; societal aspects, 308f.

Gregariousness, first-borns and (letter), 1100

Groups

dynamics, 214f.; delinquent subcultures, 419f.; democratic leadership, 366f.; discussion, 208f.; effects of leadership, 282; individual and, 193-6, 383 (letter); reinforcement and social behavior, 441f.; small, 363f.; social performance and image, 432-4; status and esteem, 178-80; union-management relations, 171f. See also Interpersonal relations, Organizations, and Social psychology

Guessing, psychology of, 274f, Guidance, see Counseling

Handicapped

deafness, 174f.; rehabilitation, 166f. Hearing, deafness and, 174f.

History

great psychologists (forthcoming book), 396; Russian-East German psychology, 406f

Hostility, emotional maturity and, 311f. Hospital, child in, 56f. (film), 132 Hypnosis

age regression, 70 2; modeling, 116f.; therapy, 76f.

Identity

anxiety and, 180f.; in extreme situations, 313-15; search for, 312f.

Incentive, reinforcement and, 184f.

Individual

conformity in industrial society, 244-6; group and, 193-6; organizations and, 48f. Industrial psychology

automation and worker, 94; human side of enterprise (letter), 254f.; job satisfaction, 125f.; physiological reactions of worker, 372f.; social scientists and, 261f.; union-management relations, 171f.; utilization of scientific manpower, 280f.; worker motivation, 9f., 65-8; worker, union and management, 453f.; Yugoslavian, 316f.

Information theory, statistics and, 68f.

Inheritance, polygenic, 337-9

Interaction potential, 178-80

INTRUCTIONAL MEDIA

anxiety in infant (film), 56f.; audiovisual equipment, 381f.; auto-instructional program in algebra, 411-13; autoinstructional program in analysis of behavior, 378-81; auto-instructional program in computer arithmetic, 25-7; auto-instructional program in English, 133-6; auto-instructional program in statistics, 104-9; child in hospital (film), 56f.; computers and thinking (film), 252; emotional development in monkeys (film), 27; international bibliography on children and cinema, 457f.; police and disturbed persons (film), 186-9; programing auto-instructional materials, 25, 104, 251f. (film), 319-21; support during childbirth (film), 56f.; teaching machines, 186 (film), 319-21; workbooks in statistics, 284-6

Intelligence, experience and (forthcoming book), 43

Interpersonal relations

dynamics of marriage, 405f.; first-borns (letter), 110f.; group dynamics, 214f.; human relations in industry, 261f.; in psychoanalysis, 147f.; interaction potential, 178-80; nurse and patient, 215f.; patient-therapist, 82f.; presentation of self, 432-4; psychotherapeutic interview, 400f.; psychotherapy, 161-3; social perception, 300f.; teacher-pupil, 436f.

Interviewing, psychiatric, 400f.

Indement

choice and (forthcoming book), 74; suspended, 149-51; perception and reasoning, 121f,

Language

communication and, 248f.; developmental psychology of cognition (forthcoming book), 43f.; development in children, 20; learning psychological German, 205; philosophical analysis of, 430f.; speech disorders, 392f.; style in, 435f.; thinking and, 246f.

Law, pornography and, 309f.

Leadership

effects on group, 282; great man, 235f.; group discussion, 208f.; group dynamics, 214f.; military, 77f.; organizational behavior, 178-80. See also Groups, Industrial psychology, and Interpersonal relations

Learning

aging and, 114f.; anxiety and, 247f.; begging of animals, 391f.; children's speech, 20; eductional psychology text (letter), 383; educational and social, 422f.; logic and, 75f.; perception and reasoning, 121f.; perceptual processes, 98f., 423-5; personality, 33-6; psychological German, 205; rats and reinforcement, 184f.; social, 199f., 357f.; Spence's theory, 371f.; S-R-X model, 275f.; structured, 356f.; symbolic processes and, 358-61; text in educational psychology, 218; theory, 33-6, 75f., 222f. (letter). See also Auto-instructional materials, Behavior, and Education

Letters, see On the Other Hand

Linguistics, readings in (forthcoming book), 74

Literature

erotic realism and obscenity, 309f.; psychoanalysis and, 10f.; style in language, 435f.

elementary structures, 197f.; learning and, 75f.; of science, 46f.

ego, sex and, 38f. (letter), 382f. (letter); sex attitudes and, 249f.

Management

automation and, 94; industrial, 65-8; scientists and, 280f.; worker, union and,

Manpower, scientific, in industry, 280f,

Marriage, dynamics of, 405f.

Mathematics

auto-instructional program in algebra, 411-13; auto-instructional program in computer arithmetic, 25-7; child's conception of geometry, 175-7; factor analysis, 374f.; introductory statistics, 68f.; quantitative methods in psychology (letter), 191. See also Models

Maturation

embeddedness affect and activity affect, 1-4; central nervous system and behavior, 425f.; personality, 80f.; physical and emotional, 23f.; process, 451f.; vocational, 276f.

#### Measurement

achievement testing, 227f.; introductory statistics, 68f.; perceptual thresholds, 98f.; principles of testing, 81f.; psychological scaling, 206f.; Q-sore in psychotherapy (letter), 60; student progress, 367f.; values (letter), 110. See also Statistics

#### Medicine

fee guide, 128; practice in mental hospitals, 168f.; surgery and stress, 408-10 Memory, dynamic and genetic theories, 70-2

#### Mental health

adjustment, 399f.; basic research, 293-6; child rearing (letter), 60; community resources, 306-8; culture and, 7f.; functional and dynamic concept, 163; national survey, 263f.; nursing and, 215f.; popular conceptions, 397-9; scholastic achievement and, 271f.; sex attitudes and, 249f.; social psychiatry, 339f. See also Adjustment and Counseling

#### Mental hospital

clinical medicine in, 168f.; out-patient treatment of schizophrenia, 448f.

#### Mental retardation

in children, 301f.; rehabilitation of subnormal, 452f.

#### Mental tests, see Testing

Metamorphosis, developmental, 1-4

#### Military psychology

civil-military relations, 77f.; game theory and conflict, 426f.

#### Mind

mind-body problem, 124f.; open and closed, 170f.

#### MMPI handbook, 167f.

#### Models

brain, 225-7; Communist party, 231f.; computers, 5f.; factor analysis, 374f.; learning, 75f., 275f.; political behavior, 202-4; statistical, 229-31; systems engineering, 260f.

#### Motivation

assessment, 22f.; child psychiatry, 177f.; drive and, 91–4; experience and (forthcoming book), 43; opinion, communication and (forthcoming book), 298; perception and, 98f., 145–7, 423–5; scoring (letter), 58f., 110; theory, 177f.; worker, 9f., 65–8; workingmen's wives, 42

Movies, international bibliography on children and, 457f.

Myth, of creation of woman, 87f.

Nationalism, personality and, 123f. Nervous system

behavior and, 425f.; biochemistry of

neurological disorders, 47f.; brain model, 225-7; localization, 151f.; models, 5f.

Neuropsychology, see Physiological psychology

#### Neurosis

anxiety, 76f.; family, 118-21

#### Nursing

mental health and, 215f.; psychology in, 183f.

#### ON THE OTHER HAND

Attempted suicide, 60f.; author-reviewer disagreements, 414f.; Body image and personality, 28f.; colleagues as reviewers, 223; Contemporary theories and systems in psychology, 58, 142, 191, 223, 287; Critical incidents in psychotherapy, 30, 191; Dimensions of transference in psychotherapy, 60; dissatisfaction with reviews, 415f.; Ego in love and sexuality, 253f., 382f.; Experimental psychology, 287; fact and value in reviewing, 287; favorable and unfavorable reviews, 61; Formulations of the person and the social context, 415; Human side of enterprise, 254f.; justice in reviewing, 30; Learning theory and behavior, 222f.; Natural child rearing, 60; obscure publishers, 111; Persuasion, 28; place of theory in psychology, 222f.; Prediction and outcome, 253; Private practice in clinical psychology, 254; Psychoanalytic education in the United States, 416; Psychology in teaching and learning, 383; Quantitative methods in psychology, 191; relevance in reviewing, 110; Rigidity of behavior, 223; Schizophrenia, 30; Scoring human motives, 58f., 110; Social psychology, 383; Statistical theory, 110; The child, 59i.; The measurement of values, 110; The powerful consumer, 3221.; The psychology of affiliation, 110f.; Toward understanding human personalities, 142, 414f.

#### Operations research, 260f.

#### Opinion

communication and (forthcoming book), 298; effects of mass media, 394; open and closed mind, 170f.

#### Organizations

Communist, 231f.; control of behavior, 48f.; individuality and conformity, 48f.; industrial, 65-8; leadership, 178-80, 235f.; military, 77f.; scientists in industry, 280f.; social science in industry, 261f.; theory, 299f.; union and management, 171f., 453f. See also Groups

Parapsychology, William James and, 149-51 Parents

delinquents and, 173f.; education for child rearing, 169f.

Party, political, 202-4

Pavlovian psychology, Soviet psychotherapy and, 12f.

#### Perception

as adaptation to environment, 451f.; basic processes, 98f.; brain injury and somatosensory functions, 272f.; chemical senses, 239f.; color discrimination, 181f.; effects of learning, 98f.; motivation, 145-7; person, 36f., 40f.; reasoning and, 121f.; Rorschach and theory, 420-2; social, 300f.; subject-centered and object-centered, 1-4; theory of development, 423-5

#### Personality

adjustment, 399f.; aging and, 114f.; autonomous, 313-15; basic tendencies, 80f.; belief systems, 170f.; body image and (letter), 28f.; child and society, 164; cognitive behavior, 404; conceptual systems and (forthcoming book), 128; culture and, 7f.; current theoretical status, 154-6; dreams and, 44; emotional maturity, 311f.; emotion and, 289-91; experimental psychodynamics, 36f.; foreign affairs and, 123f.; human (letter), 142; learning, 33-6; longitudinal studies of child, 354; mental health and, 163; MMPI assessment, 167f.; motivation, 22f.; organization and structure, 450f.; person perception, 36f.; projective techniques with children, 365f.; psychoanalytic study of child, 52f.; research on (letter), 414f.; Rorschach and theory, 420-2; social class and child rearing, 89f.; theory (letter), 415; workingmen's wives,

#### Personnel psychology

job satisfaction, 125f.; worker and automation, 94

#### Persuasion (letter), 28

Phenomenology, psychology and, 361f.

#### Philosophy

analysis of language, 430f.; bases of statistical theory, 304f.; dualism and consciousness, 238; Freud and Dewey, 228f.; logical positivism, 88; mind-body problem, 124f.; Zen and Yoga, 291f.

#### Physiological psychology

biochemistry and neurological disorders, 47f.; biological factors in mental illness, 69f.; brain injury and somatosensory functions, 272f.; brain model, 225-7; cerebral dominance, 340f.; chemistry of mental illness, 442-5; constitutional factors in crime, 86f.; effects of drugs, 84; endocrine factors in maturation, 23f.; epilepsy, 233-5; estimative system in emotion, 289-91; exercise, 240f.; human work in Yugoslavia, 316f.; industrial physiology, 372f.; Lashley's neuropsychology, 151f.; motivation and drive, 91-4; neurobiology of aging, 113f.; reticular

arousal system, 145-7; visual defects after brain wounds, 446f.

Piaget

developmental psychology of (forth-coming book), 396f.; psychoanalysis and, 351f.

Pictures

M. L. J. Abercrombie, 122; Franz Alexander, 245; F. H. Allport, 194; Edith L. Annin, 327, 329, 330; Magda B. Arnold, 290; W. R. Ashby, 226; Loren Baritz, 262; D. C. Barnlund, 209; D. E. Berlyne, 146; J. E. Birren, 115; E. G. Boring, 326, 329, 330, 331, 335; Angus Campbell, 203; J. P. Chaplin, 418; P. E. Converse, 203; W. N. Dember, 100; Sheila Feld, 263; J. L. Fuller, 338; Robert Glaser, 320; Gerald Gurin, 263; F. S. Haiman, 209; N. R. Hanson, 259; Patricia Hatfield, 263; A. Hoffer, 444; G. C. Homans, 441; E. M. Jellinek, 349; T. S. Krawiec, 418; Margaret A. Lennox, 234; W. G. Lennox, 234; B. D. Lewin, 266; S. M. Lipset, 318; A. S. Luchins, 18; Edith H. Luchins, 18; A. A. Lumsdaine, 320; Douglas McGregor, 66; W. E. Miller, 204; O. H. Mowrer, 35; P. H. Mussen, 387; J. C. Nunnally, Jr., 397; M. K. Opler, 8; Humphry Osmond, 445; W. Van O. Quine, 430; Robert Reiff, 73; Helen Ross, 266; D. G. Ryans, 436; E. G. Schachtel, 3; Martin Scheerer, 71; Robert Schlaifer, 230; Claude Shannon, 252; D. C. Stokes, 204; W. R. Thompson, 339; Clark Tibbitts, 309; Joseph Veroff, 263; P. H. Wolff, 352; Benjamin Wolstein, 148; Wilfried Zeller, 24

Police, disturbed persons and, 186-9 Politics

Communist organization, 231f.; foreign affairs and personality, 123f.; game theory and conflict, 426f.; social bases, 317f.; voting behavior, 202-4

Pornography, law and, 309f.

Positivism, logical, 88

Probability, subjective, 229-31, 274f.

Processes, symbolic, 358-61

Programing

auto-instructional materials, 186 (film), 251f., 319-21; computer, 5f., 25-7. See also Auto-instructional materials

Projective techniques

children, 365f.; French, 232f.; Rorschach and blind diagnosis, 438f.; Rorschach and psychological theory, 420-2; Rorschach study of child development, 298f.

Propaganda, content analysis and, 152f.

analysis of interview, 400f.; chemistry of mental illness, 442-5; child, 101f., 177f.; drug therapy and, 454-6; epidemiology of mental illness, 109, 306-8, 445f.; schizo-

phrenia, 448f.; social, 7f., 339f.; Soviet, 12f.; techniques, 210f.

Psychical research, see Parapsychology

Psychoanalysis

concepts of depression, 456; current approaches, 198f.; countertransference, 147f.; dreams and personality, 44; education of analysts, 265f., 416 (letter): ego, love and sexuality, 38f., 253f. (letter), 382f. (letter); existentialism and, 244-6; family neurosis, 118-21; Federn and Freud, 369f.; Freudian concepts, 160f.; hostility and emotional maturity, 311f.; human values and, 158-60; literature and, 10f.; myths of creation of woman, 87f.; Piaget and, 351f.; Schachtel vs. Freud, 1-4; search for identity, 312f.; stress and identity, 313-15; stress and surgery, 408-10; study of child, 52f.; techniques, 210f.; theory, 118-21; Western society and, 244-6

Psychology

contemporary theories and systems, 4f., 58 (letter), 142 (letter), 191 (letter), 222f. (letter), 287 (letter); economics and, 216f.; experimental control, 279f.; experimental design, 279f., 281, 282f.; experimental texts, 128f., 189f., 287 (letter); Festschrift for E. G. Boring, 325-35; Festschrift for Heinz Werner, 451f.; great psychologists (forthcoming book), 396; holistic, 451f.; in Encyclopedia of science and technology, 297f.; introductory texts, 136-40; paperbound books, 341-7; phenomenology, 361f.; readings, 310f.; reprints, 354; Russian books, 62f.; Russian-East German text, 406f.; Slavic books, 111f.; systems and theories, 417-19; text for nurses, 183f.

Psychopathology, see Behavior deviations Psychopharmacology

effect of drugs, 84, 278f.; mental drugs, 268; psychiatry and, 454-6

Psychophysiology, see Physiological psychology

Psychosis, constitutional factors in, 69f.

Psychotherapy

analysis of interview, 400f.; child, 39f.; communication in, 82f.; counseling, 269-71; critical incidents (letter), 30, 191; drugs, 268; epileptics, 233-5; family, 118-21; first-borns and (letter), 110f.; in schools, 102; modeling, 116f.; non-verbal communication, 116f.; operational values in, 161-3; progress in, 242f.; psychoeducational therapy, 11f.; scoring of motives (letter), 58f., 110; Soviet, 12f.; speech disorders, 392f.; survey, 16; techniques, 210f.; transference (letter), 60. See also Clinical psychology, Psychiatry, and Psychoanalysis

Reading, silent, 220f.

Reasoning, unconscious inferences and, 121f. Rehabilitation

deafness, 174f.; mentally subnormal, 452f.; physically handicapped, 166f.

Reinforcement

correlated, 184f.; decremental and incremental, 33-6; learning and, 145-7; performance of rats, 184f.; social behavior, 441f.; Spence's theory, 371f.; stimulus, response and, 275f.

Report-writing, 236f.

Research methods

assessment of motivation, 22f.; child development, 78–80, 385–8; epidemiology of mental disorders, 109; experimental design, 279f., 281, 282f.; experimental psychology, 128f., 189f.; human behavior, 46f.; mental health, 293–6; microscopic interview analysis, 400f.; operations research, 260f.; psychotherapy (letter), 60; quantitative (letter), 191; social work, 362f.; statistical, 304f.; values, 363f.

Retardation, see Mental retardation

Rigidity, set and, 18f.

Rorschach test, see Projective techniques

psychological books, 62f.; psychotherapy in, 12f.

Satisfaction, job, 125f.

Scaling, psychological, 206f.

Schizophrenia social therapy, 448f.; survey (letter), 30

conceptual foundations, 257-9; directory of leaders in, 354f.; discoveries in, 244-6, 257-9; education (letter), 142, 356f.; encyclopedia, 297f.; experimental control, 279f.; guide to, 127; logic of, 46f.; parapsychology and, 149-51; psychoanalysis and, 158-60

Sensation, chemical senses and, 239f,

Seriation, classification behaviors of children and, 197f.

Set

perception and, 98f.; response, 81f.; rigidity and, 18f.; theory, 75f.

Sex

attitudes toward, 249f.; creation of woman, 87f.; ego, love and, 38f., 253f. (letter), 382f. (letter); erotic realism, 309f.; premarital dating, 90f.; puberty rites, 87f.

Slavic books in psychology, 111f.

Smell, chemoreception and, 239f.

Social psychology

acquisitiveness in American society, 200f.; adolescent citizenship, 212f.; antisocial and criminal behavior, 86f.; anxiety and identity, 180f.; autocracy and democracy, 366f.; child and society, 164; class and child-rearing, 89f.; college students' attitudes toward sex, 249f.; community resources for mental health, 306-8; current theoretical status, 154-6; delinquent subcultures, 419f.; demographic factors and mental health, 263f.; dynamics of discussion, 208f.; dynamics of marriage, 405f.; game theory and conflict, 426f.; group dynamics, 214f.; individual and group, 193-6; in nursing, 183f.; interactionist, 199f.; leadership, 178-80, 235f.; mass communication, 394; person and society, 180f., 393 (letter); Polish text, 357f.; political man, 317f.; pornography and law, 309f.; premarital dating, 90f.; presentation of self, 432-4; reinforcement and behavior, 441f.; research methods in social work, 362f.; research on small groups, 363f.; research on values, 363f.; rehabilitation of subnormal, 452f.; social epidemiology of mental illness, 445f.; socialization in organizations, 48f.; social perception, 40f., 300f.; social psychiatry, 339f.; social therapy in schizophrenia, 448f.; societal aspects of aging, 308f.; society and school, 422f.; text, 199f.; theory (letter), 415; voting behavior, 202-4; Western society in transition, 244-6; workingmen's wives, 42. See also Communication, Groups, and Interpersonal relations

Social science, in industry, 261f.

Sociology

Communism, 231f.; ecology of mental disease, 37f.; political man, 317f.; premarital dating, 90f.; social gerontology, 308f.; social psychiatry, 339f.

Space, child's conception of, 175-7

Speech

development in children, 20; disorders, 392f.; silent, 220

Sports, physiology of, 240f.

Statistics

auto-instructional programs, 104–9; experimental design, 281, 282f.; factor analysis and, 374f.; introductory, 68f.; philosophical bases, 304f.; research on human behavior, 46f.; subjective probability, 229–31, 274f.; theory (letter), 110; workbooks, 284–6

Stimulus

conditioning, 145-7; phenomenological view, 361f.

Stress

industrial, 372f.; personality and, 313-15; surgery and, 408-10

Structure

collective reality, 193-6; elementary logical, 197f.

Suicide, attempted (letter), 60f.

Symbolic processes, learning and, 358-61 Systems

brain, 225-7; conceptual (forthcoming book), 128; engineering, 260f.

Taste, chemoreception and, 239f.

Teaching

audio-visual equipment, 381f.; characteristics of teachers, 436f.; disturbed children, 11f.; educational psychology text, 218, 383 (letter); mental health and scholastic achievement, 271f.; mentally retarded children, 301f.; perception and reasoning, 121f. See also Auto-instructional materials, Education, and Learning

achievement, 227f.; children with cerebral defects, 13f.; French projective techniques, 232f.; MMPI handbook, 167f.; principles of, 81f.; psychological diagnosis, 376; reporting on, 236f.; student progress, 367f.; test anxiety in school children, 247f.

Textbooks

auto-instructional, 25–7, 104–9, 133–6, 378–81, 411–13; experimental psychology, 128f., 189f., 287 (letter); introductory psychology, 136–40; paperbound, 341–7; readings in psychology, 310f.; Russian-East German text, 406f.; systematic and theoretical psychology, 417–19; writing, 127f. See also specific topics

Theory

cognition and clinical inference, 389-91; contemporary psychological, 4f., 58 (letter), 142 (letter), 191 (letter), 222f. (letter), 287 (letter); decision, 229-31, 274f., 363f.; Freud and Dewey, 228f.; game theory and conflict, 426f.; learning, 33-6, 371f.; learning and symbolic processes, 358-61; motivation, 91-4, 117f.; organizations, 299f.; perception, 423-5; personality, 154-6, 415 (letter), 450f.; phenomenology, 361f.; Piaget and psychoanalysis, 351f.; Piaget's developmental psychology (forthcoming book), 396f.; psychoanalytic, 1-4, 118-21, 198f., 369f.; psychological systems and, 417-19; Rorschach and psychological, 420-2; scientific discovery, 257-9; social behavior, 154-6, 441f.; Soviet psychotherapy, 12f.; Spence's behavioral and learning, 371f.; statistical, 229-31, 304f.

Thinking

computers and, 5f., 124f., 252; conflict and arousal, 145-7; developmental stages, 70-2; language and, 246f.; perception and reasoning, 121f.

Transference

in psychoanalysis, 147f.; measurement of (letter), 60

Translation, mechanical, 5f.

Unconscious, before Freud, 238 Unions, worker, management and, 455f.

Values

measurement (letter), 110; operational, in psychotherapy, 161-3; psychoanalysis and, 158-60; research methods, 363f.

Variables, collative, 145-7

Vision

color discrimination, 181f.; defects after brain wounds, 446f.; development of perception, 423-5

Vocational maturity, 276f.

Werner, Heinz, Festschrift for, 451f. Work, motivation for, 9f., 65-8 Writing

reports, 236f.; textbooks, 127f.

Yoga, 291f.

Zen Buddhism, 291f.

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